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Certainly it is excellent discipline for an author to feel that he must say all he has to say in the fewest possible words, or his reader is sure to skip them; and in the plainest possible words, or his reader will certainly misunderstand them. Generally, also, a downright fact may be told in a plain way; and we want downright facts at present more than any thing else.—RUSKIN.

Original Communications.

MALARIAL FEVER IN CHILD-BED.

BY THEOPHILUS PARVIN, M.D.

Puerperal Malarial Fever was the title of an able and instructive paper by Dr. Fordyce Barker, published in the American Journal of Obstetrics, April, 1880. To many this paper was a revelation, but to those whose practice for years had been in malarial regions it was the recognition of a familiar fact. My friend Dr. Hibberd, for example, now a veteran in the medical ranks, has told me that he had known the fact that puerperal women sometimes suffered from attacks of malarial fever, almost from the beginning of his medical practice. Dr. T. B. Cox, of Frankford, Ind., has informed me that for many years he has been in the habit of giving puerperal women, if confinement occurred at a season of the year when malarial disease was prevalent, prophylactic doses of quinia.

Shortly after the publication of Dr. Barker's paper, a claim of priority was made for Dr. Manson, as having many years before described the disease in question. I then wrote a short communication for the Virginia Medical Monthly, the journal in which Dr. Manson's claim had been set forth, suggesting that

priority belonged to neither Dr. Barker nor to Dr. Manson, and further showing that Burns, 1828, had written of "remittent fever" among the diseases of lying-in women, that Butter, 1775, had given "An Account of the Puerperal Remittent Fever," at least this was the title of the paper as it appeared in the Sydenham Society's publication, 1849. Other facts were adduced tending to prove, if not proving, that the profession had not waited until the latter half of the nineteenth century to learn that puerperal women might suffer from malarial disease. Whether I succeeded in establishing the point made or not, whether the proofs of that point were conclusive or not, I propose now making good my assertion by other authorities than those then adduced.

My attention has been recently again called to the subject by a Paris thesis, 1882, entitled *Etude clinique sur les accès de fievre palustre survenant apres l'accouchement*, wherein the author, Dr. Billon, attributes the first recognition of the disease to Béhier, 1858, who, in the memorable discussion in the Academy upon puerperal fever, pressed the importance of diagnosing between phlebitis and attacks of malarial fever, holding that the latter were returns of the disease awakened by the depressing influence of pregnancy and of labor upon the economy. But, as will presently be shown, Béhier has no just claim to this honor.

In the very able article by Stoltz upon *Puerperalité, Nouveau Dictionnaire de Medecine et de Chirurgie Pratiques, tome trentieme*, Paris, 1881, this eminent authority speaks as follows: "That the *encientes* and the *accoucheés* are especially predisposed to true intermittent fever has been observed by a great number of authors. Already Doublet has spoken of it in saying that he has frequently observed intermittent fevers in the *accoucheés* of Vaugirard. Berndt, in his treatise on fevers, maintains also that the newly delivered are quite predisposed to febrile paroxysms. We have ourselves many times made the same observations at Strasburg, where, however, intermittent fever is endemic. It is especially during the convalescence from puerperal affections that we have often noticed quotidian and tertian intermittent fevers, but we have never seen puerperal fever take this tpye."

In a previous part of this article Stoltz remarks that Fr. Benj. Osiander published in 1787 an account of intermittent puerperal fever which he had witnessed in 1781, but that he was in error, as is seen upon reading his observations, he has taken the *redoublements de fievre pour des accès.*

Upon consulting Doublet's little volume, *Nouvelles Recherches sur le Fievre Puerperale*, Paris, 1791, I find no reference to intermittent fever after labor, though its frequent occurrence in pregnant women is stated. *

Nevertheless, the following passage from Doublet, who, as will be readily seen, belonged to those whom the late Dr. Charles D. Meigs was wont to refer to as "milk-men," because of their puerperal pathology, is introduced as furnishing the nearest approach which I have been able to find in the volume to the confirmation of Stoltz's statement: "Sometimes, as Puzos says. *le lait repandu* takes the character of an intermittent fever. M. Beaussier, whom we have already cited as having inserted in the Journal of Medicine an observation concerning *un lait repandu* with infiltration of the thigh, remarked that this malady terminated by an intermittent fever. We have seen several times developed this character of intermittent fever in women newly delivered, and in nursing-women who had milk edemas. We have employed there with great advantage aperients and purgatives very diluted in a bitter decoction, and we have terminated the treatment by the use of cinchona and rhubarb. I mix thoroughly two drams of cinchona and one dram of rhubarb, which I have divided into packets of six or twelve grains; the patients take twelve of these packets a day. This mixture is so much more to be recommended as it has been adopted by Eng-

* An instance of intermittent fever causing miscarriage is given by Lamotte in his *Traitè des Accouchmens*, 1726. In the preface to this best of all works upon clinical obstetrics, a book which the obstetrician of the present day can so often consult with pleasure and advantage, the wise author remarks that the observations, or reports of cases as they would now be called, are of more value than the *reflexions* or conclusions he has conjoined with them; that whereas the latter change, the former are fixed, firm, and for all time. O that this master in reporting cases had more imitators! Our periodical literature would be greatly enriched.

lish and French physicians. M. Leake has used it with success, and M. Planchon has seen its good effects." After repeatedly examining this passage, it still seems to me doubtful whether Doublet referred to malarial fever.

I think, too, that Stoltz has misread Osiander, as an extract from the latter's *Beobachtungen Abhandlung und Nachrichten*, etc., Tubingen, 1787, will show. Osiander, in describing one variety of child-bed fevers as "a cold fever," stating that he gives it this designation to distinguish it from "the hot, or burning fever described by Hulme and others," observes as follows: "This fever differs from the common cold or intermittent fever which sometimes attacks women in child-bed, or with which they often pass from pregnancy to the lying-in, and which according to Torti is always very dangerous, but which can generally be cured by the use of the Peruvian bark, in this respect: that in the time between the attacks a real abatement in the feverish pulse can never be perceived, and that the chill never occurs at a regular or definite time, returning frequently, however."

Whatever may be the conclusion as to whether Stoltz has correctly given Osiander's views, there can be no dispute as to the latter's plain recognition of malarial fever in child-bed.

Let us now go back still further. The first edition of Torti's great work, *Therapeutice Specialis ad Febres Perniciosas*, was published at Venice in 1709, but that which I shall quote is the fifth, and was issued in 1755. Torti states, page 310, that although he has observed a hundred times the bark given to pregnant women, he has never seen any injurious result, nor could he even suspect one. After considering this topic at some length he says: *Quod dixi de pregnantibus, cum proportione dico de puerperis, abortum passis, et menstruas purgationes actu patientibus, quarum nulli denegari potest usus corticis, dummodo aliunde indicetur, etc.*

The index refers to three "puerperæ happily cured by bark of a subcontinuous acute fever." In mentioning the state of one of these patients, in whom the fever had caused premature labor,

Torti says: The pulse was very rapid, exceedingly small and without force, the greatest dryness of the tongue and of the fauces, insatiable thirst, almost constant vomiting of porraceous matter, the greatest aversion for food, some wandering of mind, frequent respiration, *infelixque decubitus.*

Certainly in the light of the facts now brought forward it must be admitted that malarial puerperal fever was known long before any American contributions were made to the subject; its distinct recognition belongs to the eighteenth, probably to a still earlier date, rather than to the nineteenth century. Let medicine advance, but let it not ignore the labors of those long since dead: all wisdom and knowledge are not of the present. Cicero has wisely said: *Nescire quod antequam natus esses factum sit, id semper esse puer.*

Having ended this historical review, a few words as to the recognition and treatment of the disease may be added.

That the puerpera living in a malarial region is quite liable to suffer from malarial fever, the quotidian and tertian types being oftenest observed, can not be doubted. Sometimes, too, there occurs in her that which Verneuil has called the superposition of fevers, malaria being complicated with septicemia; fortunately when this confusing condition obtains the latter is generally slight, though it may continue for several days, greatly protracting puerperal convalescence.

The means of diagnosis given by Osiander, and which have been quoted, remain of great value; the information afforded by the thermometer is also of the greatest importance. In the majority of cases there is no great difficulty in deciding whether the patient be suffering from malaria or from septicemia; nevertheless there are others where the physician must delay his opinion or accept a doubtful diagnosis. But in the doubtful cases, and in those where a complication such as referred to occurs, quinia, or rather quinina, as our new Pharmacopeia makes us say, is important, in the one condition as an antiperiodic, in the other as an antipyretic. Sometimes the patient can not take quinina by the mouth, and then let it be given by the

rectum; under these circumstances ten grains of quinina with one grain of tartaric acid may be dissolved in half a teacup of warm water, ten drops of laudanum added for a rectal injection, and the dose repeated every three hours until she is well cinchonized, when the interval should be lengthened, but she must be kept under the influence of the medicine for at least forty-eight hours. If the quinina be given by the mouth, let the dose be six or eight grains, and the intervals as before.

This plan seems to be better than that advised by Billon in his thesis. He states that the mild cases of malarial fever in child-bed, eighty centigrams to one gram of the antiperiodic is to be given in two doses in the twenty-four hours, one in the morning the other in the evening; in severe cases, one or two grams may be given, while the worst do not need more than three or four grams in twenty-four hours.

Dr. Barker has spoken highly of Warburg's tincture, but, as a rule, I have not found it well borne by the puerpera; something if not tasteless at least less disagreeable in taste, and less likely to offend a sensitive stomach, is preferable.

INDIANAPOLIS, IND.

NECROSIS OF THE TARSAL BONES, FOLLOWED BY AMPUTATION OF THE FOOT.

BY T. B. GREENLEY, M. D.

On the 10th of February, 1882, I was called to see Elmer, a son of Mr. Frank Smith, of Hardin County, this State, aged fifteen years. The history of the case up to this time, as related by his mother, was about as follows: Some time during the summer of 1881 he jumped from the stable-loft and slightly sprained the ankle of the left leg, but complained very little of it. During the fall, in plowing, he complained of his ankle hurting, and would have to sit down and rest it. Some time

in November the pain grew worse and he took to his bed. The joint soon began to swell and become intensely painful, when Dr. J., of West Point, was called to see him. The symptoms at first simulated very much those of rheumatism, and the case was so regarded both by the doctor and parents. During the winter an abscess formed and was allowed to discharge itself, in doing which sinuses were formed and several openings around the joint resulted. When I saw him pus was present between all the tarsal bones; in fact you might say the foot was dislocated and turned inward. The bones were loose and separated or movable upon each other.

I wanted to remove the foot at this time, and got the consent of the patient and his mother, but his father strenuously objected and wanted me to resort to some means by which the foot could be saved. I told him there was no way to save the foot, and its loss offered the only chance by which the boy's life could be saved. Still he objected and asked me if I could do nothing for the boy aside from amputation. I remarked the only thing I could do was to open the sinuses about the joint and let out the matter, but this would not save the foot. To gratify him I did this under anesthesia.

Four days later I was sent for with the understanding that I could take off the foot. With my friend Dr. Applegate to assist me, I amputated the foot about three inches above the ankle-joint. This was as low as we could operate, on account of the inflamed and cicatrized tissue about the joint.

The condition of the boy's health was such as almost to contra-indicate an operation. He was nearly worn out with hectic or irritative fever resulting from the long-continued ulcerative process in the bones of the foot. He was extremely emaciated, not weighing much over half his ordinary weight. His leg was greatly edematous from obstruction to the circulation, being nearly twice the size of its fellow; and to still further add to the difficulties he had come, on account of great suffering, under the morphia habit. His mother had for some time been giving him this drug from six to eight times a day, and it ap-

peared for a while as if he would have convulsions when it was withheld. His cries for it were distressing to hear.

He reacted from the effects of the operation very finely considering his weakened condition. We used as an anesthetic a combination of chloroform and ether, which I have found less depressing in debilitated patients than chloroform alone.

Owing to the weakened state of the circulation, together with the edema of the limb, I made my flaps laterally and only closed the anterior two thirds of the wound by sutures, leaving the posterior third open for drainage. I remained with the patient until thorough reaction was established.

February 16th: Elmer is doing well. Pulse 112, temperature 101.5°. Stump looks well and union of flaps nearly entire as far as they were closed by sutures. Discharge healthy. Patient has some appetite. Diet, milk with a little brandy.

February 18th: Was astonished to find patient free of fever to-day. The stump presents in front, just above the junction of the flaps, an unhealthy appearance, as if there was a tendency to slough. Boggy to the touch, giving the sensasation as if a semi-fluid substance was below the surface. I was now fully assured of what I apprehended at the time of the operation, that I would have to contend with sloughing of the stump, and perhaps have to re-amputate. I dressed the stump with ung. petrol. and oxide of zinc and kept it wrapped in antiseptic cotton wool.

February 20th: To-day the evidence was very clear that a slough at the site of junction of flaps in front, extending an inch above, would soon take place. The patient was still clear of fever and seemed to be doing well except the wound. Continued same dressing. Internally gave him quinine and iron, which he has been taking from the first.

February 23d: Slough complete to-day, leaving a space about the size of a half dollar, with the bone exposed nearly an inch in front. The margin of the opening caused by the slough does not present as healthy a condition as I could wish, the granulations appearing flabby and large. Sprinkled a little

iodoform over the sore and continued same dressing. The other portion of the stump doing well. The patient's general condition favorable. Has no fever, though pulse 90, which has been about its rate since fever left him. Appetite sufficiently good. Continued milk and brandy, quinine and iron.

February 25th: Condition of both patient and sore about same as at last visit. Continued treatment.

February 27th: Very little change since last visit. Think a part of the periosteum at site of slough is destroyed, and may have trouble on that account to get the granulations to take hold. The end of bone is now covered with new flesh, but the granulations are too flabby. Applied solution of sulphate copper. Other treatment continued. Discharge has lost its fetid odor.

March 1st: Wound closing slightly, and granulations look better. Continued treatment.

March 10th: Wound improving; granulations more healthy and now cover the bone. The discharge for several days has been more healthy. Patient gaining strength and some flesh.

March 18th: Still improving; granulations somewhat exuberant. Touched them with pencil sulphate of copper. A small scale of bone protruding through the granular surface. Treatment continued.

April 1st: Still improving; wound nearly closed. Patient's general health much improved. Treatment continued.

April 10th: Wound about closed. Patient can sit up; has good appetite and gaining flesh. Giving general directions for his future management, dismissed him.

I have not seen the boy since my last visit, but hear that he is up on his crutches and doing well in every particular.

Remarks. It may be asked why the case was allowed to progress so far as to endanger the boy's life before surgical aid was afforded him? This question may be partly answered by the resolution of the patient himself not to have any one beside myself to perform the operation. This determination upon his part grew out of the fact that the family were formerly my

neighbors, and he was better acquainted with me than any other physician, and as I was in New York at the time he concluded to wait for my return home. Besides his father was, as before stated, violently opposed to the operation.

I publish this condensed account of the case, not on account of the infrequency of necrosis of the bones of the foot, but more particularly to show under what untoward conditions we may sometimes operate and save the life of the patient.

I have never seen a patient apparently suffer more pain than this boy before the operation was performed. His mother sat by him nearly day and night, and was compelled to keep him under the influence of morphia. We found it necessary to continue the use of this drug more or less for several weeks after amputation, partly on account of the habit and partly to procure rest.

As far as can be ascertained there is no family history to lead us to suspect any blood dyscrasia by which the necrosis was produced, and the inference is drawn that it resulted from an injury produced by jumping from the stable-loft.

The above report was written in June. The boy is now, December 2d, entirely restored to health, in fact more robust than ever before, and has an excellent stump.

OREL, KY.

CARBOLIC-ACID INJECTIONS IN HYDROCELE, RANULA, AND CYSTIC TUMORS.*

BY P. E. SANDIDGE, M.D.

The purpose of the present paper is to give an account of a few cases of hydrocele, ranula, and analogous tumors, which I have treated in the last dozen years by injections of carbolic acid.

In March, 1868, I saw Mr. W. with my late lamented friend

* Read before the Cumberland County Medical Society, May, 1882.

Dr. J. W. Beauchamp, sr. The case was one of double hydrocele. Being led to think of carbolic acid as a probable cure for hydrocele by reason of what I had witnessed of its effects, in the year or two preceding, in promoting the healing of wounds, I suggested its use as an injection in this case instead of the tincture of iodine, or port wine, etc.; but, Dr. B. objecting, I emptied the sacs and threw in the tincture of iodine after the usual manner. The patient did well, and the operation seemed to be a success.

Two years later the vaginal tunic again began to distend, and in April, 1871, I was called to operate a second time. The tunic of the right side was greatly distended with a dark-looking fluid, which I drew off with an ordinary trocar and canula, and injected two or three drams of a solution of Calvert's carbolic acid No. 5, one part of water to three of acid. I manipulated the walls of the sac in such manner as to bring the solution in contact with their every part, and then slowly pressed it out. The contents of the left tunic were clear. Having withdrawn them through the canula, I threw in two drams of tincture of iodine, kneaded the parts, and withdrew the injection as on the right side. Two hours later the patient suffered intense pain and heat, with retraction of the testicle of the right side. The scrotum was much swollen and red, and the penis became erect. Rigors soon followed, and these in turn by fever. The left side underwent no change. A brisk purge or two, rest in bed, low diet, and cold lotions to the inflamed parts straightened matters, and in ten days the case was dismissed. The right testicle remained for a time somewhat retracted, and the scrotum on that side considerably corrugated. There were no marks of trouble on the left side.

Nine months after (January, 1872) the left tunic had refilled. The right was cured. I emptied the sac, injected carbolic-acid solution, and got a speedy cure. The patient, now alive, has never had the slightest return of his complaint.

Mr. H., a farmer, thirty-five years old, who had a hydrocele succeeding upon an attack of abscess of the epididymis, was

cured by a single injection of carbolic acid. The patient died six years after with consumption, without having had a return of his hydrocele.

From April, 1871, I have treated a considerable number of cases of hydrocele by the simple method here mentioned, with uniform success. I have not had to use the injection more than once in a single case. It has also come in my way to relieve a number of subcutaneous cystic tumors by the same means. I have had equal success with carbolic-acid injections in the treatment of ranula. The following report of a case may be interesting:

In November, 1876, I saw, with a medical friend, a girl thirteen years old, otherwise in good health, having the largest ranula I had ever met with. It positively hung under her chin like the pouch of a pelican. It pressed the tongue up against the roof of the mouth, interfering with articulation, deglutition, and respiration. Here, as in the first case of hydrocele I mentioned, I yielded to the wishes of my medical friend, and operated in the usual way, using tincture of iodine as the irritant. The following April the sac was more distended than ever before. I at once withdrew the fluid through a canula introduced at the most dependent portion of the tumor, and injected six ounces of Calvert's solution of carbolic acid No. 7, and took care to bring it in contact with every part of the cyst before it was withdrawn. The parts quickly became red and intensely painful. Rest in bed was enjoined, cold applications with moderate pressure were made to the parts, and a saline purgative given. Nothing further untoward occurred, and the sac has never refilled.

I have treated two cases of ranula since that time by the same method, with like success.

BURKESVILLE, KY.

Reviews.

The Diseases of the Liver, with and without Jaundice, with the Special Application of Physiological Chemistry to their Diagnosis and Treatment. By GEORGE HARLEY, M. D., F. R. S., Fellow of the Royal College of Physicians; Corresponding Member of the Academy of Science in Bavaria, of the Academy of Medicine of Madrid, and of several Continental Medical Societies; formerly President of the Parisian Medical Society; Physician to University College Hospital; and Professor in University College, London. Illustrated by colored plates and wood engravings. Philadelphia: P. Blakiston, Son & Co. 1883. Pp. 751.

The motto of this volume is, "True science is the key to true practice," which is good; and the dedication is an "In Memoriam," being a posthumous revival, with an addendum, of an inscription made twenty years ago to the then living Dr. William Sharpey, by the author, of a monograph, the progenitor of the present work, which is loyalty to the shades of a departed patron.

No mention is made in the title that this is a second edition of the work, but the first line of the preface advertises that the author, twenty years ago, published a monograph on jaundice; and it is presently stated that this volume, although it embodies "within it the whole substance of my original monograph on Jaundice and Diseases of the Liver, bears no more resemblance to it than a mature adult does to the suckling from which he sprung." This shows how enormously a liver-doctor expands mentally in twenty years, and at the same time indicates the propriety of the curb the author puts himself under to make a *brief* extract from his hepatic knowledge to constitute this volume; for if a brief statement occupies seven hundred and fifty-one pages, what a monster treatise a full statement would be!

The first reading-matter in this book is an announcement, on

the fly-leaf facing the title-page, that through a special arrangement between the author and the American publisher the work is issued simultaneously in England and the United States, which is a neat and effective way of impressing the average medical man with the importance and value of the work; and after an examination of the contents of the book, comprehending what the author says, and mentally triangulating the vast plains of his knowledge that he hints at, the impressible reader is entirely ready to rise and, with the classical Dominie Sampson, exclaim, p-r-o-d-i-g-i-o-u-s!

Dr. Harley undoubtedly possesses a large fund of valuable information touching the disorders of the liver, but he mixes it up with so much irrelevant, incongruous, egotistical, and contradictory matter that the heterologous mass is too gross for profitable assorting; in fact he mounts the theme as a hobby and rides it with a loose rein and a reckless disregard for the paths he ought to pursue; now dashing off into fields of speculative hepatology, and again loitering over plains that have not a spear of hepatology of any kind on them. When it comports with his mood he demands the most rigid adherence to scientific facts and logical deductions; where scientific exactness does not lead to his goal he becomes the most credulous of garrulous polemics.

With him chemistry, physiological and pathological, is the keystone to the arch of scientific medicine, and he condemns the colleges because their curriculums do not embrace it sufficiently; he rails at teachers because they do not more thoroughly instruct in it; he insists that after the student is perfect in all other branches he should devote three years to practical laboratory study of it; and finally he covertly intimates that there is but one man who has completely mastered the subject, and gently leads the earnest inquirer to infer that this phenomenal man is named Harley. Perhaps, in the fresh special current language of the laity, the author might be understandingly characterized as a learned and enthusiastic hepatological crank.

To sustain the frankly-expressed opinion of the character

of the author, and in some measure to illustrate the substance, manner, and style of his book, some liberal extracts from the volume are submitted, even their length to be construed as emblematic of the author's prolixity. The first is under the head of "Dietetics of Hepatic Disease," beginning on page 170:

As champagne plays a not unimportant part in the treatment of all the more exhausting forms of liver-disease, I shall take the present opportunity of ventilating my views on this universally appreciated vinous beverage, for I wish to change, if possible, the present pernicious English habit of drinking sour wine, disguised under the name of *champagne sec*, under the mistaken notion that it is wholesome wine which has become naturally dry with age, while in reality it is no such thing, but *tastes dry* simply because it is *sour*. I say sour, for the various degrees of dry, very dry, and extra dry (*sec*, *tres sec*, *et brut*) champagnes are simply wines of different degrees of acidity—sourness. If any reader doubts this, let him for himself make the experiment of dipping a piece of blue litmus test-paper into his fine (?) dry wine, and (if not already aware of the fact, which in all probability he is not, or he would not have a single drop of the liquid within his doors), I promise he will open his eyes wide with astonishment at the tint the paper will assume. Vinegar—pure, strong wine vinegar will not bestow upon it a brighter red tint; and why? Simply because the flavor which he ignorantly imagined is due to "dryness" is, on the contrary, due to the presence of acid. And the secret is simply this: "Dryness," as it is absurdly called, is the product of age. It is in fact due to the slow transformation of the sugar in the bottled wine into alcohol—as takes place in the twenty, thirty, and forty-year-old port. But it does not pay the wine merchant to keep his champagne till its saccharine matter has been transformed by fermentation in the bottle into alcohol, and the sweetness of the wine has consequently disappeared; so he adopts the speedier course of getting rid of the sweet flavor of the wine by setting up the quick acetous instead of the slow alcoholic fermentation, which has the effect of destroying all the saccharine matter contained in the bottled wine in a few months. Or he adopts another equally effectual course of adding less than four instead of, as he ought, eight per cent of syrup to the wine at the time of the "dégorgement." In fact there are many ways well known to the trade of "spoiling" champagne to suit the ignorant, depraved taste of the English consumer. I say English, for no nationality has as yet been found foolish enough to swallow sour wine under

the delusion that it is drinking good, sound, dry wine, from finding the word *sec*, *tres sec*, or *brut* on the labels of the bottles.

Of course some of my readers will think this very strong language, but let me tell them that not a syllable of it is too strong; and if any one of them doubts the truth of what I say, and thinks he knows a deal more about the matter than I do, let him take the trouble to make inquiries of a champagne wine-merchant—not an ordinary English wine-merchant, for most probably he will be as ignorant of the whole matter as the reader is himself, but a French wholesale champagne wine-dealer—and he will soon discover that every word I have said is not only perfectly true, but not even exaggerated. The following anecdote will show the ideas of a continental champagne dealer regarding the Englishman's knowledge of good champagne:

All my champagne I import myself, and, as my personal friends know, it is A 1. The gentleman who I in general deal with comes to England occasionally, and on one occasion, when he called upon me and got an order, I observed that in booking it he wrote after the name of the wine the word CONTINENTAL, and not only so, but carefully underlined it. On seeing him do this with an air of the most perfect *sang froid* I exclaimed, in a voice of surprise, "Why have you written the word 'continental' after champagne? Is all your champagne not *continental?*" To this he immediately replied—and that, too, with an air of sweet innocence—"O, no; we never now send any continental wine to England." "What on earth do you send, then?" exclaimed I in breathless astonishment. He smiled and answered, "Spoilt English champagne." Being more bewildered still, I slowly repeated his words, "Spoilt—English—champagne. What do you mean?" "O, Dr. Harley, don't you know that all the dry champagne is specially prepared for the English market? We can't sell a single drop of it on the Continent, for nobody will drink such stuff. It's quite sour." Seeing my consternation, I presume, he quickly added, "We never send it to you. Your champagne is what we drink ourselves. It's true champagne—none of the *tres sec* stuff." "But," said I, "all good champagne is slightly acid on account of the carbonic acid it contains." "Yes, that is perfectly true; but it's not sour, which so-called champagne is. You're a chemist. The first time you have the chance dip a piece of blue litmus paper into the two wines, and you will soon see the difference. While the continental one will yield a faint pink, the English one—that is, the sour, which you call dry wine—will immediately turn the paper crimson, just as sulphuric acid would." Here was a piece of important infor-

mation for me; and now I shall proceed to give a little further information, which may perhaps prove almost equally interesting to the reader.

The very next day I was called by Dr. Macaldin to see an old rich bachelor suffering from a violent bilious attack, accompanied not only by vomiting, but diarrhea also. On finding that it was brought on by his having made a hearty dinner of *a pound of salmon steak* and a bottle of champagne, without partaking of any thing else; and my wine-merchant's information regarding "spoilt English champagne" running in my head, I mildly asked if the champagne partaken of was *sec* champagne. The prompt reply was, "O, yes, the very best *tres sec* that can be bought. It could do me no harm." "I am not so sure of that," said I. "Perhaps the wine is sour." "Sour?" said he. "I never drink common 40s. trash. My champagne stands me 84s. per dozen, and is as dry as an old bone and without a particle of acidity in it." "Are you quite sure of that?" said I. "Yes, perfectly sure. You may take a bottle of it home with you and test it." "If you will allow me," said I, "I'll test it here. I have a piece of test-paper in my pocket, and it will soon tell us whether the wine is sour or not." "Then do so," said he. "But wait a minute. You shall have a clean glass and a fresh bottle." While this arrangement was being put in execution I took out the litmus paper from my purse, walked across to a side-table upon which I saw the cruet-stand, took out the vinegar-bottle, and nearly half filled the wineglass with its contents; then poising a piece of blue litmus paper between my finger and thumb, said, "Do you see this? Now watch the result." The end of the paper was plunged into the vinegar, held there for a second or two, then withdrawn. "There, you see, it has turned blue to red; that shows that vinegar is sour." "Of course," replied our confident patient; "but that's sour vinegar, not fine old dry champagne." The butler had brought in a fresh bottle and a couple of clean wineglasses for Dr. Macaldin and me. Pop went the cork; gurgle-gurgle went the sparkling, effervescing, fragrant wine into the glasses. I took one of them up in my hand, held it straight out in front of me, close to his bedside, and quietly waited until all the froth had disappeared from its surface, in order to make sure that the chief part of the carbonic acid had escaped. Then poising, as before, a fresh strip of blue litmus paper between my finger and thumb, I looked significantly at the sanguine, smiling face of the patient, and, with marked deliberation, quietly and slowly dipped the strip of paper deeply into the wine, let it remain there for a few seconds, and then as deliberately

and slowly withdrew it. What was the result? Nothing more nor less than that, when it was placed side by side with its fellow that had had been dipped into the sour vinegar, both were found to be nearly equally crimson in color. The patient's face no longer wore a smile. It was now as proportionately long as it had before been broad. His eyes stared motionlessly; he gazed into vacant space. Not a syllable escaped from the lips of either of us. Then suddenly, as if a bright thought had struck him, he shouted to the servant to bring him his writing-desk. It was soon placed beside him on the bed and immediately opened, when, after rummaging about in it for a little time, forth came a piece of folded paper, and, tossing it to me, he, with a gruff voice, exclaimed, "Read that." I took up the paper. It was a receipted bill. I read it over, and from it learned the to me interesting fact that our patient's *tres sec* champagne was put down at 90s. a dozen, and that 6s. a dozen had been deducted for cash payment; so that the sour trash which he had ignorantly consumed as "dry wine" had, as he said, cost him 84s. per dozen. This proved a memorable night to him; for, as Dr. Macaldin afterward told me, the little chemical experiment I performed upon the beautiful dry (!) wine had kept the poor patient awake the whole night. He could not get a single wink of sleep, for the colors blue and red and the words *sec* and sour, as he said, haunted him like a nightmare. I have only to add that from that day to this I believe this gentleman has followed my example, as many other of my friends have done, and left the drinking of *tres sec* to those happy mortals who enjoy a life in the fool's paradise, where ignorance is bliss, and turn their poor stomach's into pickle-jars while drinking different grades of wine vinegar under the sweetly seductive titles of *sec*, *tres sec*, and *brut* champagnes, specially manufactured for their depraved tastes. . . .

In the eye of the champagne-trader the word *brut* simply means a wine that has been left to itself to undergo fermentation; but this, which might appropriately be called natural champagne, is poor and sour; so that even the so-called natural *brut* champagne has to undergo a process of "doctoring" at the period of *dégorgement* to suit it for the English palate. Hence even it also is artificial. In fact all refinements of human taste are simply artificial acquirements which have become popular and at length got gradually stereotyped by fashion. In proof of this I shall now show how the most disgusting drinks and foods are made by fashion not only palatable but exquisitely delicious! The Fijii Islanders, for example, drink with *goût* and intense delight what appears to us in the light of a disgusting concoc-

tion, called "cava" or "kava," which Admiral Sir Henry Denham tells me, from his own personal observation, is prepared as follows: The women sit down in a circle and chew the root of the *Piper methysticum*, spitting the juice, mixed of course with their own buccal secretions, into a bowl placed in the center of the group. Water is then added in proper proportion and the mixture allowed to stand and ferment for two hours, at the end of which time it is ready for drinking.

To us this appears a loathsome beverage, but to the Fijiians it is a "dainty dish fit to set before a king;" and is in reality set before their king, for he has a great dishful of it brewed all for himself. (A huge specimen of these kava-bowls may be seen in the museum of the Kew Botanical Gardens. It is much deeper and nearly as long as an adult's coffin.)

So much for funny artificial drinks. Now for even a more strange kind of artificially-manufactured food. In Kane's Tour in North America I came upon the following receipt of the Chinook tribe of Indians for the preparation of a savory dish of olives: A hole is dug in the ground near the entrance of the family mansion, the hut. About a bushel of the finest acorns are then put into it, covered over with a layer of grass, and then the hole filled in with about a foot thick of earth. Now the work of cooking begins. From this time henceforth, for the next four or five months at least, every man, woman, and child in the family urinate into the hole, and in due time the acorns become saturated and softened, pungent and odoriferous, and are then partaken of as one of the finest of all earthly delicacies. Such at least is the opinion of the refined-tasted Chinook Indians. Can we then feel the least surprised that the educated, highly civilized—though in wine-knowledge ignorant—Englishman should equally prize and relish sour champagne? He is only to be pitied, not to be blamed any more than the Fijii Islander or Chinook Indian. On the wise philosophic principle, be it said, that what is one man's meat is another man's poison, we find the British matron priding herself on the nice flavor of her high (*putrid!*) game, the Spanish lady on that of her *rancid* salad-oil, and the German *hausfrau* glorying in the *geschmach* of her stinking cheese (*handkāse*).

In conclusion, then, my advice is, when ordering champagne for an invalid, to tell him to get wine that is neither labeled *sec*, *tres sec*, nor *brut*, but wine such as is drunk in France, which is not "sweet champagne," such as is sent to Russia, but really good, wholesome, palatable wine.

Perhaps an apology should be made for this long quotation, which after all is only a part of the author's side treatise on wine; but it is so characteristic of one phase of the author's eccentricity as to be paradigmatic, and the reader is petitioned to excuse its extent upon the score of its intrinsic value, and for the further reason that it discloses the state of the author's mind on the points of condensation and brevity, for he assumes that his book is conscientiously shorn of all useless thoughts and unnecessary verbiage in these words (preface, p. xviii):

> Moreover, as I still think time is quite of as much importance to the professional as to the mercantile man, I have endeavored to condense my materials to the utmost without running the risk of endangering their perspicuity. . . . I shall not waste time by entering into detailed accounts of the literature, nor give tedious and probably at the same time profitless discussions, . . . but limit myself entirely to a brief exposition of my own views.

Let us remember, however, in this connection, as a plea in extenuation, that champagne, whether *tres sec* or continental, sometimes has the effect on doctors to both confuse their ideas and induce a diarrhea of words.

A second great quotation will serve to disclose the scientific attainments of our author in diagnosis, and at the same time evidence the manner in which he meets his *confrères* and demonstrates their inferiority—viz. thus on page 478:

> On October 2, 1877, a celebrated Q.C. consulted me about the nature of what he called an "inveterate indigestion." He told me he had consulted four physicians, among whom he named Drs. Andrew Clarke and Murchison, and that they all agreed that it was a simple case of "functional derangement." I listened most carefully to all he said, examined his abdomen to the best of my abilities, but found nothing—not even so much as an iota of a suspicious physical sign; yet I could not reconcile the "inveterate indigestion" with the idea of "mere functional derangement," perhaps for the simple reason that "functional derangement" as a disease occupies no place in my medical nosology. At any rate I acted according to my invariable principle, which is, when in doubt analyze the urine. The urine was

brought. I analyzed it and found therein a trace of melanin! The diagnosis was made—"malignant disease of the stomach." It is said that one swallow does not make a summer, but such a line of reasoning does not hold good in disease. To my way of thinking the undeniable existence of one single cancer cell, be it where it may, or the tiniest trace of melanin, is proof positive of the existence of malignant disease, which a million of negatives can not gainsay. This has been proved to me many times, but seldom or never more conclusively than in this case, unless it be in one other, equally striking if not more extraordinary, where a few cancer cells in the urine gave a correct clue to the nature of a most obscure case of cancer in a dignitary connected with Westminster Abbey, whom I frequently saw in consultation along with Mr. Holthouse, and who was also seen by Drs. Bence Jones and George Johnson, as well as by Mr. Prescott Hewitt.

The Q. C., whose case I am now referring to, expected shortly to be raised to the Bench, and he asked me to be plain-spoken to him and hide nothing. Accordingly I was so, and honestly told him that with care he might live a year or two, but without care he would not live six months. He thanked me, and by my advice went back to Dr. Murchison (who happened to be the physician who was then prescribing for him) with a note from me telling him of my having found melanin in the urine, and explaining to him how to test for it. Back came the Q. C. in a week, somewhat sarcastically saying, "How you doctors differ! not only in matters of opinion, for that would be nothing, but in actual matters of fact. Murchison has three times examined my urine, and he could n't find a vestige of melanin in it, though you said that you found it at once. He says that with all your chemical knowledge you have mistaken altered urine pigment for melanin, and that instead of my having a deadly disease about me he will cure me in three weeks."

Too well, alas! I knew not only the value to be attached to the promised cure, but that the cause of Dr. Murchison's not finding melanin arose from his confounding the reaction of so small a quantity with that of urohematin; therefore I expressed to the patient no opinion regarding Dr. Murchison's remarks, but simply said, "As he says that he can cure you in three weeks, and I feel sure that I can not cure you at all, go back to him and let him treat you up till New Year's day — that is, six weeks from now — exactly double the time which he says will suffice for your cure, and, if you are not cured by that time, come back and put yourself unconditionally under me. In

any case come and see me on New Year's day, and I sincerely hope to be able to congratulate you on Murchison's success." He said he would follow my advice, and so we parted; but I never saw him again till February 15, 1878, three days before his death, when he sent for me, as he said, in order to receive my forgiveness for having broken his promise to me. Our interview was to me a most painful one, his whole conversation being a narrative of blighted hopes and vain regrets at having put faith in the promised cure, at the same time telling me how he had been kept buoyed up with hope until exactly three weeks before that day, when Murchison informed him that he had a cancerous tumor of the pylorus. I put my hand to the spot indicated, and there, sure enough, was a tumor as large as a cricket-ball.

This, then, is another case illustrative of the vast importance of introducing science into the domain of practical medicine. Be it noted, too, that the patient lived only four and a half months from the time that I said with care he might live a year or two, and without care not six months.

This extract establishes the fact that pathological chemistry in its highest development enables its apostle not exactly to cure cancer, but to publicly put a slur upon a rival practitioner and to publish a correct prophecy as to how long a patient will live—after he is already dead.

A third grand quotation will exhibit the extraordinary skill with which the author conducts certain manipulations and how handsomely he tripped up the distinguished Sir Henry Thompson by his superior *tactus eruditis*. Indeed the author has been singularly successful in his consulting associations with the more prominent medical men of his day. This volume narrates a number of such meetings where he held opinions not in accord with his associates; and it is remarkable, and much to the exaltation of his acumen, that in every instance the result approved him to be in the right. But to the quotation, page 433:

On a Sunday forenoon in the month of November, 1864, one of my out-patients at University College Hospital brought his son, aged thirteen months, to my house, suffering from intense bladder irritation. The father and grandfather having both suffered from stone,

and the visible signs manifested by the child (he could not yet talk, so no information could be obtained from him), coupled with that fact, clearly pointing to the existence of a vesical calculus, and as I make it a rule never to follow the reprehensible practice of prescribing for a stone without first having ascertained its true nature when such a thing is possible, I determined to examine the child's urine before prescribing for him. But as the little fellow was either unwilling or unable to make water, I had no alternative but to draw some off. So taking a small "infantile" silver catheter I passed it into his bladder and drew off as much water as I required; then, before withdrawing the catheter, I made a search for the stone. Almost at once the end of the instrument touched it. Guessing that it was small and could be very easily removed at one sitting by a lithotrite, and the child thereby instantly relieved of its suffering, I sent father and child along with a note to my then colleague, Sir Henry Thompson, with a request that he would kindly operate on the patient. To my surprise the father brought back the child to me with a note from Sir Henry Thompson, saying that he had sounded the child's bladder, and that I was quite mistaken, for there was no stone whatever in it, and consequently the symptoms must be due to something else. Having more confidence in the value of stone symptoms than in my colleague's infallibility, and feeling, too, that it was almost impossible for me to have mistaken something else for the peculiar sensation the contact of the stone communicates to a metallic catheter, I no sooner read the note than I ordered the man to replace the child on the sofa and reintroduced the catheter. Again I detected the stone; and now, wishing to find out its size, I passed the forefinger of my left hand into the child's rectum and guided the stone toward the point of the catheter. (In children, from the parts being small and the tissues thin, a great deal may not only be learned, but done, by manual manipulation.) Finding that I had the stone firmly between my finger and the point of the catheter, and the idea having struck me that I could possibly push the stone into the urethra by guiding it thither with the catheter, a trial was made, and the trial was successful. In less than three minutes I had the satisfaction of having the stone firmly lodged in the urethra, and within ten minutes more I had manipulated it through the exterior walls, always keeping it following the catherer, up the urethra to within half way between the scrotum and point of the penis. Here it stuck, and I was just about prescribing a powerful diuretic, in order to get the stone floated out, when it occurred to me that it might perhaps be as well to let my

colleague not only see that physicians may detect calculi when surgeons fail, but kill two birds by one stone, and get him to cut it out with a knife from where it then was.

So I kept the child quiet on the sofa while the father went back and asked Sir Henry to come round and bring a bistoury with him. In a few minutes he appeared; but he brought no bistoury, probably thinking that none would be required. I placed the little catheter in in his hand, and told him that if he passed it gently into the child's urethra he would discover a stone. Before allowing him to do so, however, I secured the stone in its position by compressing the urethra immediately behind it with my finger and thumb, for fear it might accidentally slip back into the bladder, upon being touched with the point of the catheter, before he had time to satisfy himself of its existence.

As soon as he expressed himself satisfied that a stone was actually in the child's urethra, I handed him a bistoury and requested him to cut down on it and let it out. He did so, and the stone is now safe in my calculi collection—to me a not uninteresting souvenir.

These excerpts do not savor to any great extent of hepatic affairs, but nevertheless they fairly limn the author's mental make-up, as well as the variegated solid and frothy substance of his work. There is not wanting, however, abundant hepatological material in the book. Early in its contents Dr. Harley enumerates thirty-four causes of jaundice, and four hundred pages later tabulates thirty-nine pathological conditions that attend it, and specifies eight—and says there are others—abnormal structural alterations of the liver not necessarily associated with jaundice. All these states, he affirms, can be accurately and differentially diagnosed by the qualified practitioner; and he supplies in the last chapter of his book six aphorisms and seventy-eight principal and eighty subsidiary rules, which will unerringly lead him who strictly follows them to this high estate. The nature and worth of these instructions must be estimated by the reader from the character of the author as he gleans it from the specimens of his writing so liberally furnished in this review, which is already too long to allow of further elucidation of the author's peculiarities. Indeed the writer knows of no justification of the

length of this review as it stands except that the book, being ostentatiously heralded as springing into existence in two hemispheres at the same moment, implies that it is a work of abounding merit, and the readers of this journal are entitled to such an exhibit of its salient points as will enable them to judge, in some measure at least, of the justice of its claims from seeing something of its strength and much of its weakness, and by this token determine whether or not they wish to purchase and study the work. J. F. H.

The Pharmacopeia of the United States. Sixth Decennial Revision. New York: William Wood & Company. 1882.

From the preface it appears that seventy-eight crude drugs, twenty inorganic drugs or chemicals, one hundred and six pharmaceutical preparations, and seventeen miscellaneous substances found in the last Pharmacopeia are dismissed from this; while, on the other hand, there have been added to this thirty crude drugs, one hundred and fifty pharmaceutical preparations, sixty inorganic drugs or chemicals, and sixteen miscellaneous substances. Obviously, so far as number is concerned, the gains are more than the losses.

"To supply a demand which has arisen for dry, powdered extracts, a new class of preparations has been introduced under the title of Abstracts." Medical journalists, as well as their readers, know very well what is meant by abstracts, and real-estate dealers know what abstracts of titles are, but now come the druggists and pharmaceutists asserting another meaning for abstracts.

Possibly these gentlemen will claim retrospect, and put that in the next Pharmacopeia. Another change is the introduction of the metric system in the working formulæ.

These, however, are minor matters; a much more serious thing is the change in the strength of some of the important

preparations; thus, as pointed out by Dr. H. C. Wood (Philadelphia Medical Times), the tincture of opium and the deodorized tincture have the quantity of morphia in each increased one third.

A sin of omission is that Churchill's tincture of iodine, used by at least one doctor in five, is not given; while this invaluable preparation is omitted, we have menispermum, oil of sesamum, and a dozen like remedies that not one doctor in a hundred ever thinks of using; the santoninate of soda is given, which is probably the most disagreeable form of administering santonin, and at the same time the most useless.

In some respects the sixth decennial revision of the Pharmacopeia disappoints us; so many changes made, some of them quite radical, drugs and preparations admitted that might be well omitted, and some omitted that ought to be admitted, and changes in the strength of several preparations without, as it seems to us, sufficient reason. In fact, we seriously question if revision will meet the approval of the profession. Probably when the next revision is made the profession will take more interest in the matter, and have more to do with the reception or rejection of remedies.

A Practical Treatise on Diseases of the Uterus, Ovaries, and Fallopian Tubes. By A. COURTY, Professor of Clinical Surgery, Montpellier, France. Translated from the third edition by his pupil, AGNES M'LAREN, M.D., M.K.Q.C.P. With a preface by J. MATTHEWS DUNCAN, M.D., LL.D., F.R.S.E., etc. Philadelphia: P. Blakiston, Son & Co. 1883.

The first edition of this work was published in 1866, and, procuring it then, it has been one of the most frequently consulted books in our library. Some time after the appearance of the first edition a New York publisher advertised that he would issue a translation, but so far this publication has not been made.

Certainly no one could translate the work better than this Scotch lady has. We can use no expression too strong in commendation of the admirable translation made by Dr. Agnes M'Laren. We regret that diseases of the vagina and of the external sexual organs have been omitted in the translation; for students and practitioners, generally buying but one book on diseases of women, desire that book to be complete.

The first chapter of the work is occupied with the anatomy, physiology, and teratology of the organs of generation. A good foundation is thus laid for the study of diseases of these organs. The volume is then divided in two parts—the first entitled A General Survey of Uterine Disease, and the second Uterine Diseases in Detail.

One marked and valuable characteristic of Courty's work is the broad view taken of uterine pathology and therapeutics. The author recognizes more distinctly and fully, probably, than most writers on maladies of females, the important part which diatheses and constitutional states play in their genesis and in their continuance. While, in some respects, possibly too much of a localist in therapeutics, yet, as a rule, he gives great prominence to constitutional treatment.

One thing which will strike the American physician as quite remarkable is, that Courty makes no reference to laceration of the cervix and Emmet's important operation for its cure. Indeed, among Courty's operations for dysmenorrhea he describes one as "autoplasty by the formation of artificial commissures," in which the resulting os, figure 242, page 317, looks very much like a representation of lacerated os, almost a so-called stellate laceration. We doubt very much whether a patient on whom Courty has performed this operation could live in some parts of the United States a year without the apparent lacerations being pared and stitched; for, if there were no ectropion and no leucorrhea, there would at least be nerve fibers caught in the cicatricial tissue, and making complaint through the top of the head or the bottom of the foot, or in some other remote part of the economy—complaint which can in no way be silenced

but by that marvelously overdone operation known as trachelloraphy.

We regret to find that nearly forty pages devoted to menstruation, in the original work, are omitted in the translation. Indeed the book has undergone still other condensations than by the omissions referred to. Nevertheless it remains one of the best books on diseases of women, and the American profession may be congratulated upon its publication in this country.

Clinic of the Month.

A Few Words about Eatables. — The following is an abstract of a supposed dialogue between Dr. C. B. Radcliffe, of London, and a layman at the breakfast-table — the latter having eaten largely of meat, while the doctor confined himself to a little galantine, butter, and brown bread. To save space we omit the talk of the layman:

Lean meat is not the only kind of nitrogenous food which will serve your purpose. It is impossible to distinguish between the albuminose or peptone into which fibrine is resolved in the process of digestion, and the albuminose or peptone into which albumen, or caseine, or gluten, or legumin is resolved in this process. It is apparently of little or no moment whether these various nitrogenous articles of food are derived from the world of animal life or from the world of vegetable life. You must allow that a herbivorous animal is not less vigorous than a carnivorous animal; and certainly you would find it difficult to show that man, who can live and thrive under the most dissimilar circumstances upon almost any kind of food, is vigorous in proportion to the amount of meat he contrives to consume. All the nitrogenous substances, animal and vegetable, are resolvable into albuminose in the process of digestion, but not with the same facility in every case. Some of them are digested more easily by some persons than by others; and besides, there may be differences in the albuminose itself which are recognizable by chemical means. In your own case lean meat may be more digestible than any other nitrogenous compound, and the albuminose into which it is converted may be more easily assimilated. In another case eggs or cheese or maccaroni may better suit the requirements of the person taking it. I do not venture to lay down a hard and fast rule for you or any one in this matter; I only want you to understand distinctly that a person who can not get a full allowance of lean meat, or who does not choose to get it, is not necessarily ill-fed for that reason, even though he have to do hard work with his muscles. The part of the nitrogenous food

which is not wanted for plastic purposes is, after digestion, resolved by the action of the liver into urea, and the other excrementitious products which are met with in the urine, and into a compound containing carbon, hydrogen, and oxygen without any nitrogen, which compound may be the substance called amyloid substance or glycogen. This non-nitrogenous compound is destined to serve as fuel for the production of heat and other forms of force. The portion eliminated as urea, which is simply excrementitious, and the complemental portion, which is destined to serve as fuel, is as 33.20 to 66.80; and therefore it is easy to see that a large part of the nitrogenous food—but little less than two thirds, that is to say—may be devoted to other than plastic purposes, and that a little more than one third may be simply wasted. Moreover, the comparatively small portion of nitrogenous food which is actually wanted for plastic purposes is, there is reason to believe, eventually disposed of in the same way as the portion which is not used for plastic purposes, a little more than one third being wasted as urea, and a little more than two thirds being utilized as fuel. And if this be so, the question arises whether the fuel into which a large part of the nitrogenous form of food is resolved sooner or later is the best form of fuel for your purposes — whether, for example, you were wise in picking out the fat and in taking dry toast?

I think there is good reason to believe that much of the fuel, without which life can not be maintained, may be more easily supplied by non-nitrogenous substances than by nitrogenous substances. The fuel in nitrogenous food is not ready-made. This food has to be transformed, first of all, into albuminose or peptone, and then this albuminose or peptone has to be broken up, partly into the excrementitious portion which passes out of the system by way of the kidneys, and partly into the residual portion which is destined to act as fuel. An abundant supply of gastric and pancreatic and intestinal juices is wanted in order to bring about the proper formation of albuminose; without a healthy condition of liver and kidney it is evident that the albuminose may not be broken up (this breaking-up occurs chiefly in the liver) into urea and amyloid substance or glycogen, and that the urea (which passes out of the system by way of the kidneys) may not be eliminated. Moreover, it seems to be certain that no one can take a large amount of meat and other highly nitrogenous compounds for a long time unless he also do a large amount of muscular work—unless he do much more work of this sort than the great majority of human beings are willing or able to do. Fat and butter and oily matter gen-

erally, on the other hand, require no digestion in the proper sense of the word. They are converted into an emulsion—which is no more than a mechanical mixture like cream—by the action of the pancreatic and duodenal juices chiefly, and by the action of the bile partly, and this emulsion passes directly into the general circulation of the blood through the lacteals directly, without going the round of the portal circulation and the liver, as albuminose has to do. Fat and butter and oily matters generally are fuel ready made, or which only need to be emulsified in order to be in this case; and they have this advantage also—that they are burnt up in the system without leaving behind them, so to speak, any ash-like urea. And, as force-producing agents —if the capacity for oxidization may be taken as a measure—the value of fat and oil is almost double that of fibrine or albumen.

What you have to do, first of all, is to bear in mind that the daily loss which has to be made good by food in a man of medium stature and in moderate work, amounts to 4,800 grains of carbon and 300 grains of nitrogen, and that, in round numbers, lean meat contains eleven per cent of carbon and three per cent of nitrogen, and bread thirty per cent of carbon and one per cent of nitrogen. The daily rate of wasting of the system which I have mentioned is that which is brought to light by very many observations, carried on by many persons in various ways, with a view to regulate the food-rations of soldiers and sailors and prisoners and other ration-fed people; and as to the proportion of carbon and nitrogen to lean meat and in bread the evidence is sufficiently conclusive. You must mix your lean meat and bread in certain proportions if you care to feed without wasting good food. In order to replace the daily loss of 4,800 grains of carbon by lean meat, the quantity of meat you must take is 43,637 grains, or rather over six pounds—a quantity which contains 1,009 grains of nitrogen in excess of the 300 grains actually wanted. In order to replace the daily loss of 300 grains of nitrogen by bread, the quantity of bread you must take will be 30,000 grains, about four pounds—a quantity which exceeds by 25,200 grains the 4,800 grains of carbon which are actually wanted. There is no occasion for you to eat these monstrous quantities of meat or bread. You must eat six pounds of lean meat every day if you take nothing else but lean meat; you must eat four pounds of bread every day if you take nothing else but bread; but you may get on very well upon a comparatively small allowance of meat and bread if the two were combined in proper proportions. You want every day 4,800 grains of carbon and 300 grains of nitrogen; you find what you want, as Dr.

Pavy shows, in two pounds of bread and in about three fourths of a pound of lean meat, thus: 14,000 grains (two pounds) of bread contain 4,200 grains of carbon and 140 grains of nitrogen; 5,500 grains (about three fourths of a pound) of lean meat contain 605 grains of carbon and 165 grains of nitrogen; making a total of 4,805 grains of carbon and 305 grains of nitrogen.

You need not take so much, or any thing like so much bread, if you will take fat with your meat, or butter with your bread, or any oily matter in proper quantity. Fat is very rich in carbon, and so are all fatty and oily matters. You would have the 4,800 grains of carbon and the 300 grains of nitrogen which you want, if you took three fourths of a pound of lean meat and about two and a half ounces of fat. In proportion as you increase the amount of fatty or oily matter you may diminish the amount of bread; and within certain limits, which you may determine for yourself, you may probably please yourself as to the relative proportions of the two. Whether you would get on satisfactorily by excluding bread altogether, and taking fatty matter in its stead, is another question. The growing chick within the egg has plenty of oily matter to feed upon, and nothing of the nature of starch or sugar, or any other carbo-hydrate to take the place of bread. The sucking mammal finds a large amount of oily matter in the milk upon which it feeds, and a somewhat larger amount of lactine, or sugar of milk, which, as a carbo-hydrate, may more or less take the place of bread. In the hen's egg the proportion of fatty matter to albuminous matter is as 82 grains to 110 grains. In cow's milk the proportion of fatty matter to lactine is as 351 grains to 468 grains, and of these two substances in conjunction, together with caseine, as 811 grains to 369 grains. In two pounds of bread and three fourths of a pound of lean meat, the proportion of fatty matter to carbo-hydrates is as .944 ounce to 16.320 ounces, and of both these substances together to nitrogenous matter as 17.264 ounces to 4.908 ounces. By comparing the composition of two pounds of bread and three fourths of a pound of lean meat with that of eggs, you may also, I think, form some idea of the amount of fatty or saccharine matter which is necessary to replace the two pounds of bread. The nitrogenous matter of six pints of milk or thereabouts is equivalent to that of two pounds of bread and three pounds of lean meat, for in six pints of milk there are 4.082 ounces of fatty matter and 6.416 ounces of lactine; and, therefore, you may conclude that the 4.082 ounces of fatty matter and 6.016 ounces of lactine which are present in the six pints of milk are equivalent, for practical purposes, to the .944 ounce

of fatty matter and to the 16.320 ounces of starch and other carbo-hydrates which are met with in the two pounds of bread and three fourths of a pound of lean meat. The nitrogenous matter of twenty eggs is about equal to that of two pounds of bread and three fourths of a pound of lean meat, for in twenty eggs there are 1,600 grains, or 3.66 ounces of fatty matter; and therefore you may conclude that the 1,600 grains, or 3.66 ounces of fatty matter which are present in the contents of twenty eggs may take the place of its .944 ounce of fatty matter which are met with in the two pounds of bread and in the three fourths of a pound of lean meat, and of the 16.320 ounces of starch and the other carbo-hydrates which are present in the two pounds of bread. For it may be fairly assumed that the properties of the nitrogenous and non-nitrogenous compounds are as properly balanced in the egg and milk, which are two great typical forms of natural food, as they are in the artifical combination of bread and meat of which we are speaking. In the case of the egg an uncertain amount of lime, probably a large amount, ought to be added, for the shell becomes thinner and thinner as the process of incubation goes on, in consequence of the solvent action of the phosphoric acid which is generated by the oxidization of the phosphorus in the contents of the egg. In the case of white bread (white bread was used in this experiment) the greater part of the mineral matter, which is lodged chiefly in the husks of the grain, is sifted out in the preparation of the flour from which white bread is made. The earthy matter of the shell is certainly necessary to the proper development of the bones of the chick, and in all probability the bones are not the only tissues which are in this case. A dog lives long and thrives when it is fed upon brown bread, but not when it is fed upon white bread. If the body is to be properly nourished the mineral matters which are contained in the different articles of food can not be excluded, that is evident. And if these different articles of food are to be properly digested, the common salt in the food, or taken along with the food, may have a very important work to do in addition, for without it it is not easy to see how the gastric juice could acquire that part of its acidity which depends upon the presence of hydrochloric acid. You have only to consider how olive oil is used in the warm parts of Europe where the olive is cultivated, and how ghee is used in India, in order to satisfy yourself that oily matter may be taken with facility in hot countries as well as in cold. You hear nothing about indigestion; you find that a bad olive harvest or scant supply of ghee is a great national calamity. A Hindoo servant of a friend who kept up his Indian

habits of eating here in London has often told me that in his own case nothing would make up for a deficiency of ghee or butter, and that his experience in this matter was the common experience of his countrymen at home or away from home. He looked upon a sip of ghee in very much the same light as that in which his fellow-servants looked upon a draught of beer. "Wine is good, but oil is better," said a peasant to the courier who was with me the other day in Andalusia, and after gulping down a large mouthful of olive oil and smacking his lips more than once, the expression of his countenance was an apt illustration of the meaning of the Scriptural text which speaks of oil as making "the face to shine." Indeed, it may be taken for granted that oil may be used in large quantities throughout the year in the hot olive-growing countries of the south of Europe, not only without making the people bilious or out of order in any way, but with unmistakable benefit. Fat and butter and cream are heat-producing agents without doubt, but heat is only one of several modes of force which are closely correlated; and there is reason to believe that the molecular movement which gives rise to heat in one case, may, in another case, give rise to electricity or some other form of physical force. I do not believe that heat is transformed into muscular force or nerve-force. I believe that the oxidization of the force-fuel which gives rise to heat in one case, may, in the case of a muscle and nerve, give rise to the electricity which is peculiar to muscle and nerve; that this electricity antagonizes the state of action in both muscle and nerve; that in muscle it also causes elongation of the fibers during the state of rest; and that muscular contraction is brought about by the action of the attractive force which is inherent in the physical constituents of the muscular molecules when this force is no longer antagonized by their electricity. Indeed, all that I want to bring about muscular contraction is, not a metamorphosis of muscle which issues in the development of muscular force, nor a transformation of heat into muscular force, but simply a supply of electricity during the state of muscular inaction which will counteract the tendency which the muscle always has to contract as an elastic body. I want, indeed, not a special muscular force, but merely the common attractive force which is inherent in the physical constitution of the muscular molecules, and electricity to counteract the working of their attractive force when necessary. I have a notion that the beneficial action of the fats and oils is not wholly to be accounted for by regarding them merely as force-producers. I believe that they actually serve as food for nerve-tissue. This tissue is in the main made up of a peculiar kind

of fat; and I am convinced that nerve is starved if the food be wanting in a sufficient quantity of fatty or oily matter. I find that very many persons suffering from various chronic disorders of the nervous system have abstained from the fatty and oily articles of food, and that their state is almost invariably very much changed for the better when you can get them to take what they have avoided; I also find that a great number of delicate infants who can not take skimmed milk, and who do not take kindly to unskimmed milk, will take milk without any difficulty when it is enriched with cream. I prefer, as food for invalids, milk enriched with cream or some other fatty matter, or the yelks of eggs, or something like the *bouillon* of the French *pot-au-feu*, to highly nitrogenous preparations from which the fat has been carefully skimmed off, as ordinary beef-tea. Lean meat more or less fluidified, and its juices, are not the *sine quâ non* in food if what I have said be true. On the contrary, I am disposed to think that in very many cases foods of this sort are really unsuitable, if only by calling upon the liver to do work which this organ is unequal to at the time. I am quite a believer in the virtue of unskimmed milk as a most suitable food for invalids of all ages in almost all cases; and I think that in very many cases where this fluid does not agree, this difficulty will be got over by the addition of cream or some fatty matter. I can imagine that many mothers who can not feed their infants in the proper way, or get fresh cow's milk or cream, will have reason to be glad when they can procure preparations of condensed or inspissated milk enriched with various quantities of cream or some fatty matter. I can imagine that preparations more or less similar to those, which, for the reason I have just hinted at, might properly be called brain-food or nerve-food, might make cod-liver oil almost superfluous as medicine, and be of infinite service to countless myriads of persons in whom brain-power or nerve-power is lacking. I can imagine that in many cases it will be difficult to find a food for invalids which is to be preferred to lightly boiled yelk of egg, or to ordinary egg-flip. And in the cases where it is expedient to use flesh—meat in one form or another—I am sure it will be a great change for the better when, instead of having recourse to beef-tea, or Brand's Essence, or Liebig's Extractum Carnis, the thoughts are turned to something like the *bouillon* of the French *pot-au-feu*, or rather to the very thing itself. This *bouillon* is much more pleasant to the taste than broth or stock. It is the outcome of ages of experience in the people who have a special genius for cookery. The animal and vegetable ingredients are so blended that the flavor of no one article is predominant. The *bouil-*

lon contains all, or almost all, the soluble portions of those ingredients which are necessary for tissue-forming or plastic purposes, and for force-production, and when taken along with bread, it provides a meal for an invalid which is most palatable, most digestible, and most restorative. It is the basis of all good gravies and soups, becoming, for example, excellent *purée* or pea-soup when a proper portion of pea-flour is added to it. The *bouilli* can not be of any very great value as food; and I am very much disposed to think that its place may often be supplied with advantage by bread or potatoes, or some other form of farinaceous food. For myself, I should infinitely prefer a basin of *bouillon* with bread, or a basin of *purée* with bread, to a basin of *bouillon* and a plate of *bouilli* after it, without bread; and I think my instincts do not mislead me in this matter. I have a small appetite, and no superabundance of digestive power; my inclinations turn toward vegetable food rather than toward animal food, and I can easily see that farinaceous food may be really more suitable to the wants of my system than any thing which is left behind in the *bouilli*.

For making an ordinary *pot-au-feu*, Gouffé, in his *Livre de Cuisine* (Paris, Hachette, 1867), tells us to take of fresh meat about one and three fourths of a pound, fresh bones (smashed) about one fourth of a pound, leeks about seven ounces, carrots, onions, and turnips about five and a half ounces, parsnips about one ounce, celery about one third of an ounce, salt about one ounce, one clove, caramel a very little, water seven imperial pints. Having placed the meat and bones in the stew-pan, with the bones undermost, the water is poured in, and the salt added. Then, after putting it upon the fire and allowing it to remain there until the water boils, and a scum collects upon the surface, the pan is removed from the fire and the scum skimmed off, a little cold water being first added for some purpose or other which is more intelligible to a cook than to me. Then this process of boiling, adding a little cold water, removing from the fire, and skimming, is repeated twice. Then, and not until then, the vegetables are added, and the pan is placed near enough to the fire to allow the contents to simmer (not to boil) for three or four hours. Then the *bouillon* is poured off and the *bouilli* prepared as a dish in one way or another. And lastly, when the *bouillon* is in the soup-tureen, and not until then, enough caramel is added to it to give it a delicate orange tinge—*une tainte dorés*. The lid of the stew-pan is never to be closed down tightly, for if this be done the *bouillon* is very likely to spoil by becoming thick and muddy.

The quantity given here is for four or five persons. To try and

make less, Gouffé tells us, is bad economy, likely to issue in bad cookery, and this is intelligible enough, for the *bouillon* may be used in various ways, not only on the first day, but on the day following. The imperial pint, containing twenty onnces, is the pint referred to.

I think that bread may still be very properly spoken of as "the staff of life," and that other farinaceous articles of food may very properly be admitted into the same category with bread. The composition of wheaten flour—which is more or less that of all flour prepared from cereal grain (oats, rye, barley, maize, rice, and the rest), and of leguminose seeds or pulse (peas, beans, lentils), and also of potatoes and some other tubers and roots—according to Dr. Letheby, is: Nitrogenous matter, 10.8; fatty matter, 2; carbo-hydrates (starch, sugar, and the rest), 70.5; mineral matter, 1.7; water, 15. The nitrogenous matter consists of vegetable fibrine, albumen, and gluteine in the rough form of gluten. The fatty matter is in no way peculiar. The non-nitrogenous carbo-hydrates are starch, dextrine, sugar, gum, cellulose, and lignine—starch chiefly. The mineral articles comprise phosphates of lime and magnesia, salts of potash and soda and silica. Leguminose seeds or pulse contain as much as from twenty-five to thirty per cent of nitrogenous matter, mainly in a form of caseine called legumine; rice and potato contain as little as about eight per cent of nitrogenous matter, and as much as eighty per cent of starch, the amount of nitrogenous matter and starch in these articles of food being in an inverse ratio to each other. Fatty matter is especially abundant in oats and maize. It is evident, therefore, that there is much in these vegetable articles of food which may take the place of the nitrogenous and oily articles which are supplied in animal food.

There is no essential difference as to chemical composition between vegetable albumen and fibrine and legumine and oily matters and animal albumen and fibrine and caseine and oily matters; there is no perceptible difference in the albuminose or peptone into which the vegetable and animal nitrogenous substances are alike transformed in the process of digestion; there is no difference in the way in which the vegetable and animal oily matters are emulsified and then taken up directly into the general circulation of the blood. Nor is it difficult to see how the starch and sugar and other non-nitrogenous materials which are peculiar to vegetable bodies are disposed of within the system. The way in which starch is disposed of in the stomach and bowels is not very well made out, and all that can be affirmed with certainty is that a great part of it finds its way into the liver through the portal system of vessels, and is detained there for a time in the

form of amyloid substance or glycogen—a detention which is not altogether unaccountable, for, as Dr. Pavy points out, this substance "possesses diametrically opposite physical properties to sugar, being a colloid, and therefore non-diffusible, instead of a crystalloid and diffusible." There is no sufficient reason to suppose that the action of digestion, be that what it may, is always to transform the starch into sugar; for sugar in quantity could not be formed in the stomach and bowels without passing directly into the general circulation, and so out from the blood into the urine by way of the kidneys—without making, that is to say, the phenomena of diabetes a natural state of things instead of an unnatural. Nor is there sufficient reason for supposing that the amyloid substance of the liver is transformed into sugar, for this substance is as readily oxidizable and as fit for force-fuel as sugar. Nay, it may be questioned whether sugar itself is the force-fuel which the system is in need of. There is a very rapid generation of lactic acid in the stomach and bowels when sugar is taken as food, and it is not unintelligible that it should be so; for, with the help of a ferment of some sort, grape-sugar is readily converted into lactic acid. Indeed, all that has to be done is for one atom of anhydrous grape-sugar to split up into two atoms of lactic acid. Nor is it unintelligible that a certain part of the starch taken as food should pass, as it would seem to do, not into amyloid substance or glycogen, or into sugar, but first into dextrine, then into sugar, and then into lactic acid: for, as it seems in the list which I show you, there is a close chemical correspondence between these various substances and those which are akin to them. Thus: Starch, dextrine, cellulose, lignine or woody matters, and gum, carbon 12, hydrogen 10, oxygen 10; cane-sugar, carbon 12, hydrogen 11, oxygen 11; grape-sugar, and amyloid substance, carbon 12, hydrogen 12, oxygen 12; lactic acid, carbon 6, hydrogen 6, oxygen 6. There is no difficulty, therefore, in understanding to some extent how it is that, under the action of pepsine, or diastase, or some other ferment, starch and dextrine and cellulose and lignine and gum and cane-sugar and grape-sugar and amyloid substance may be transformed into the lactic acid which forms so important an ingredient in gastric juice, and that the lactic acid so formed, after having done its work in digesting nitrogenous substances, may be absorbed into the circulation directly, and be there disposed of in oxidization as a very readily inflammable fuel—perhaps as the more readily inflammable of all the force-fuels. And certainly there is no reason to believe that amyloid substance or sugar is more inflammable than lactic acid, but rather the contrary,

for lactic acid can not be traced, as amyloid substance and sugar can be, beyond the limits of the alimentary canal. In any case I am, I think, at liberty to assume that a good deal of starch and sugar, and of the articles akin to them, are of great use in supplying lactic acid, and that this lactic acid has to do very important work, not only in the primary processes of digestion, but also as force-fuel.

The effect of taking sour buttermilk, sour milk, and sour whey—the sourness of which depends upon the presence of lactic acid—is unequivocally beneficial in many cases. I have long been in the habit of recommending these articles in cases where the digestive power is feeble and the circulation wanting in vigor, and I am quite satisfied that the practice is very satisfactory in its results. Instead of being "a weight to the stomach," as fresh milk often is said to be in these cases, these drinks are generally found to facilitate digestion and to keep up the warmth of the system. Indeed, by using sour buttermilk and sour whey, I have often found it possible to leave off doses like rum and milk and to do without alcoholic drinks altogether.

I have more than once heard an Irish peasant say that he misses the sour milk he takes along with his potatoes almost as much as the potatoes themselves, and that "it warms him like whisky and keeps off the rheumatiz." I have again and again felt myself benefited by taking buttermilk. And, certainly, I find it difficult to turn a deaf ear to all that I have heard in praise of the whey-cure in Switzerland and elsewhere by those who have tried it for dyspepsia and rheumatism. I was led to recommend sour buttermilk or sour whey by reflecting on these very facts.

THE THERAPEUTICAL VALUE OF CHRYSOPHANIC AND PYROGALLIC ACIDS.—At a recent meeting of the New York Dermatological Society Dr. Morrow formulated the following conclusions touching chrysophanic and pyrogallic acids:

1. That chrysophanic acid is perhaps the most efficient agent known to the profession for the external treatment of certain cases of psoriasis, especially chronic cases which have resisted other methods of treatment.

2. That its range of application is limited; in children, in patients with sensitive, irritable skins, and in acute cases generally, it is contra-indicated.

3. That in psoriasis affecting the face and hairy scalp the intensely

irritating action, producing puffiness of the face and eyelids, and its discoloring effect upon the hair, render its employment impossible.

4. That it is prompt in its action, a week or ten days' active treatment being usually sufficient to develop its full therapeutic efficacy.

5. That its curative effect is only temporary; it does not afford a safeguard against relapses.

6. That it probably acts only locally and by virtue of its irritating properties, setting up a substitute inflammation, which modifies or corrects the tendency to the inflammatory overgrowth of epidermic cells.

7. That its employment is attended with certain objectionable results, some of which always follow its use, while others seem to depend upon idiosyncrasy, physiological and morbid predispositions, etc.

8. That a brownish, prune-juice discoloration of the skin which persists long after the application is discontinued, a reddish staining of the hair and nails, and an indelible dyeing of the clothing are inseparable from its use.

9. That the erythematous and furuncular inflammations which occasionally follow its use may be classed as incidental effects, as they do not always depend upon an excessive strength of the preparation employed, but are frequently manifest after a mild application; intense dermatitis, resulting in exfoliation of the epidermis in large flakes, has been observed after an application of ten grains to the ounce.

10. That the strength of the ointment recommended by Balmanno Squire (two drams to one ounce) is excessive; a milder strength (twenty grains, one dram to one ounce) being usually sufficient to develop the full therapeutical virtues of the drug.

11. That in other diseases for which it has been recommended, as acne, favus, pityriasis versicolor, eczema marginatum, etc., chrysophanic acid possesses no advantages over certain other drugs which are commonly used.

12. That pyrogallic acid is a drug which is free from some of the more objectionable features of chrysophanic acid. It does not (in ten-per-cent ointment) inflame the skin, it does not produce edema of the face when applied to the scalp, and the discoloration is much less marked and permanent.

13. That it should, nevertheless, be used with caution, as pernicious results have followed its too free use. When freely used for two or three weeks it produces an olive-green or tarry condition of the

urine, with prostration, febrile disturbance, and other general symptoms.

14. That its curative action in psoriasis is much less rapid, but apparently more permanent than that of chrysophanic acid.

15. That its freedom from irritation and its absence of odor render it an admirable substitute for chrysophanic acid and oil of cade in diseases affecting the scalp and face.

16. That while its effect in psoriasis is slower and less brilliant than that of chrysophanic acid, its range of therapeutical action is much more extended. It causes to disappear the nodosities of lupus, the hyperplasiæ of syphilis, epidermic and palpillary hypertrophies, and seems to have a good effect in promoting the cicatrization of wounds.

17. That it seems to act by virtue of its stimulant and irritating properties; it hardens and shrinks the tissues, shrivels up unhealthy granulations, and acts as a hemostatic.

Dr. Sherwell thought that Dr. Morrow's conclusions were so just that they would be agreed with by the majority of dermatologists. He believed that ointments of chrysophanic acid of the strength used by Squire and those who followed his advice (one half to two drams to one ounce) were entirely too strong. He himself never used it in a greater strength than ten grains to one ounce, and often prescribed seven, or even five grains to one ounce.

Dr. Bulkley said, as regards the strength of ointment to be used, he had not found that small quantities of the drug were always sufficient, having had one patient who used it the strength of three drams to one ounce before the eruption would yield. As regards pyrogallic acid in epithelioma, he had ordered the pure acid sprinkled on, and poultices afterward applied over the lesion, and had found that its effect in cutting down the epitheliomatous granulations was often wonderful. He recalled particularly one case of a large epithelioma of the temple, in which a pyrogallic acid ointment was used without subsequent poulticing, and when he last saw the patient the ulcer was almost entirely healed.

Dr. Denslow remarked that he had recently made trial of another drug in the treatment of psoriasis, in cases of tender skins, on the face and scalp, and in children, where he regarded chrysophanic acid as contra-indicated. This agent was the oil of turpentine, used at first in the strength of one part to four, or one to three, gradually increased to equal parts of the oil and olive oil. He had applied it by friction with a cork covered with flannel, as recommended by Dr.

Piffard. In many cases he has found this agent to act as well as chrysophanic acid ever does, and it neither produces staining nor irritating effects.

Dr. Piffard said that he agreed fully with sections 1, 2, 3, 5, 7, 9, 10, and 13 of the report. As regards section 4, his experience had been different from that of the author of the paper. He had found it desirable to keep up the action of the drug for a longer period, sometimes for at least one month. He usually began treatment with an ointment of ten grains to the ounce, and gradually increased to one dram to the ounce, and, as a rule, with great benefit to his patients. As to the discoloration produced, it did not seem to him that it lasts so long as intimated in the paper; it generally disappearing in from two to three weeks in his experience. The color produced was, he thought, often a beautiful Indian red, rather than brownish, or of that of the juice of prunes. As regards the use of the drug in pityriasis versicolor, the "chromophytosis" of our nomenclature, he thought it wonderfully efficient, and in so-called eczema marginatum he thought it one of our most valuable agents. He had never had any success with it in ringworm of the scalp, but had often found it very useful in ringworm of the body. The discoloration produced by pyrogallic acid he had found to be black or brownish. He did not use this agent as much as he did chrysophanic acid, having had one or two unpleasant experiences with it. With regard to section thirteen, he thought that it was not put with sufficient force. Pyrogallic acid is a dangerous drug when extensive applications of it are made. It has been found to kill dogs experimented upon with it. It is well known that it is absorbed by the skin, and that it is one of the most active reducers known to chemists.

With regard to the staining of the clothing caused by chrysophanic acid, it seemed to him that by using the pure powder, and afterward painting it over with collodion, would obviate that drawback. Scarenzio used collodion to limit the extension of the inflammation produced by the drug to the diseased patch, his method being to surround the patch to be treated with a ring of this fluid before applying the acid. He had met cases of psoriasis in which chrysophanic acid did not seem to exert the slightest effect, and other instances in which it did positive harm, causing the eruption of fresh patches of the disease in the erythematous areole which it excited around old patches. He had also sometimes noticed that it failed to remove the disease when it had relapsed, although it had caused its disappearance when used the first time.

Dr. Morrow stated that he always directed the scales of psoriasis to be removed before applying the acid, either mechanically or by the use of salicylic acid, one part in sixteen of alcohol. He desired to emphasize the statement that certain skins will not tolerate chrysophanic acid. In one case he had found that five or even three grains of the acid to the ounce developed intense dermatitis. In another case even two and a half grains to the ounce were not tolerated, the whole epidermis desquamating in large flakes when an ointment of this strength was applied.

THE INDICATIONS FOR THE USE OF DIGITALIS.—Dr. J. Milner Fothergill, in a paper published in a recent number of the Glasgow Medical Journal, says, touching the use of digitalis:

The correct use of this potent remedy—invaluable in certain cases of lack of power in the heart—is scarcely as yet general. Old established views take a great deal of uprooting; and yet they must be uprooted before new views can be built up in their place on the same ground. Digitalis was long regarded as a cardiac sedative, "the opium of the heart," because it rendered the heart's action slower or less tumultuous. Slower, certainly, in those cases where the rapidity is due to the action of an irritable muscle; irritable, because becoming exhausted. But when the rapidity of the heart's action is due to nervous disturbances the digitalis is useless, or very nearly so. Digitalis then is not useful "because it slows the action of the heart." This is an error. In many cases it exercises no action worth estimating upon the rapidity of the heart's contractions. While in others it is of the greatest service when the action of the heart is not accelerated before its administration, nor slowed while the good effects are being felt. "Less tumultuous," most certainly, in many cases. Where a heart is laboring hard, yet accomplishing little—when the muscle is doing its best to the utmost of its power, but is heavily handicapped—then digitalis will usually calm its action, not, however, by any sedative effect, but by increasing the vigor of the cardiac contractions. In other words, it may be said that digitalis achieves the more complete emptying of the ventricle at each systole; and that is what is wanted in these cases.

Now, sometimes digitalis will both slow the heart's action and do away with palpitation at one and the same time. This is most commonly seen in simple dilatation of the left ventricle, without necessarily any valvular lesion; the mitral valve may leak, but not as the

result of any distortion of the valve curtains, but rather the ostium has stretched with the yielding of the heart-muscle, and the valve curtains become insufficient to close the ostium completely on the contraction of the ventricle. Such a condition is common where the dilatation has taken place too swiftly for the valve curtains to stretch *pari passu* with the yielding of the muscle. Here digitalis is usually of priceless value. But its utility will be greatly enhanced here by putting the patient at complete rest; which means strictly confined to bed—just as much as if the case were one of broken thigh.

"Digitalis is to be given in mitral disease, but withheld in aortic disease," is a rule of thumb driven into the student's mind, like a nail into a plank, by some teachers. Well, as a broad rule it is well enough; digitalis is usually of service in mitral disease; but how about aortic disease? When a fairly hypertrophied left ventricle is struggling against a contracted aortic orifice, but not quite successfully, how about digitalis? The system is suffering for want of arterial blood because the ventricle is unequal to driving *a sufficiency of blood through the narrowed ostium in the normal time* to keep the arteries full. Here digitalis often acts most potently, indeed furnishes the most brilliant illustration of its properties. By increasing the vigor of the driving power—the ventricular contractions—the normal amount of blood is pumped into the arteries in the normal time, and tissue nutrition is improved every where, including the structures of the heart itself. Or aortic regurgitation is dilating the left ventricle too swiftly for hypertrophy to be built up to arrest the dilating process; what is the value of digitalis here? Simply inestimable. It arrests the dilating process! the ventricle recovers its size, and, with that, much of its vigor; the muscle is better nourished, and then that compensatory hypertrophy is built up which often enables the patient to pursue an active life for years.

Certainly, on the other hand, both in aortic stenosis and aortic regurgitation, while the muscular compensation is complete and sufficient, and the patient is fairly well, there is no good end to be attained by giving digitalis. We do not give digitalis because there is valvular disease present, but when the system is suffering in consequence of the said valvular lesion. The digitalis has no influence upon the injured valve. But it is of mighty service when the muscular hyperplasia, which compensates the valvular defect to a great extent, is not provided by the powers of nature. By the aid of digitalis the natural powers will often be enabled to surmount the difficulty and secure a muscular growth, or hypertrophy, which is practically compensatory.

Such compensation by muscular hypertrophy is most perfectly seen in aortic stenosis. And on this hangs the good prognosis of aortic stenosis.

It is quite clear that under these circumstances the action of digitalis is powerfully aided (1) by rest, reducing the demand upon the heart; (2) good food to aid in nutrition of the tissues; and (3) iron as a hematic. In mitral disease the effect of digitalis upon the right ventricle often leads to most satisfactory results.

Now, when we come to discuss the effects of digitalis upon the right ventricle, there is something more to be considered than the heart merely. There is the respiration! Ordinarily we breathe eighteen times per minute or thereabouts. There are about two hundred and fifty inches of "residual" air in the thorax, and the act of respiration takes place normally about eighteen times per minute. By such "tidal" air the "residual" air is kept fairly pure. But when the thoracic space is encroached upon either by (*a*) air in emphysema; by (*b*) connective tissue in cirrhosis; by (*c*) diminution of the caliber of air-tubes from thickening of the bronchial lining membrane; or (*d*) by engorgement of the blood-vessels in mitral disease, then the respiration must be more frequent in order to keep the residual air fairly pure. The stimulus to respiration is the effect of venous blood, laden with carbonic acid, upon the respiratory center in the medulla.

When there is an excess of carbonic acid in the blood circulating in this center, then the respiratory efforts are increased in vigor until the excess of carbonic acid is got rid of. Now, when the right ventricle is embarrassed, it is not usually enough to give digitalis to increase the energy of the contractions of the right ventricle. Though, of course, all medical men of much experience have met with striking illustrations of the almost magical effects of digitalis in the pulmonary engorgement of mitral disease; many also can tell of cases where digitalis failed to afford relief under these circumstances, or even increased the respiratory embarrassment. Now, my rule for some time past has been, under these circumstances of mitral lesion, no matter what form, with embarrassed respiration, to give strychnia, a well recognized "respiratory stimulant."

Here, the effect of the digitalis upon the right ventricle, and that of the strychnia upon the respiratory center, work together for good with most satisfactory results. The good effects of this combination are conclusively demonstrated in those cases where digitalis, given alone, fails to do good, but where the addition of strychnia at once makes a striking alteration. Inversely, when there exists any condi-

tion of lung or bronchiæ by which the respiration is embarrassed, or the thoracic space diminished, then digitalis may be added to the cough mixtures with decided advantage. Whenever the breathing is embarrassed and the radial pulse feeble, while the contractions of the heart are vigorous upon auscultation—a condition which tells that the right side of the heart is laboring—then digitalis may be given with a respiratory stimulant, as ammonia, or nux vomica, or both, to the great relief of the patient. Usually, that is. Of course, if there be anatomical changes which forbid real relief, then the effects are less palpable. The proper relation of digitalis to stimulants of the respiratory center is a matter not understood as generally as is desirable.

The indication then for digitalis is not a murmur in the heart, nor a certain form of valvular lesion, nor tumultuous action, nor yet rapidity of action, but, as Rosenstein has put it, whenever it is desirable "to fill the arteries and empty the veins." That is the impression which each student of medicine should form in his mind as to the action of digitalis. If he would do so, the doubts which otherwise may beset his mind in the exigencies of practice will not often embarrass him. To remember Rosenstein's axiom will serve him well many a time and oft, when in doubt as to what to do—to give or withhold digitalis. Say it is a case of aortic regurgitation: if the arterial system is well filled then digitalis is contra-indicated; but if the wall of the heart be yielding in the later stages, then surely it ought to be given. In almost all cases of mitral lesion digitalis is indicated. But there is another condition in which digitalis is sometimes given with injurious effects which contrast with these conditions. The hypertrophied gouty heart often palpitates when there is arteriole spasm, and the larger arteries are tense and full of blood. The resistance offered by this full arterial system to the onward flow of the blood at the cardiac systole is such that the ventricle palpitates in its efforts to contract effectually. Such a condition is commonly seen in the "chronic Bright's disease without albuminuria," so well described by Dr. Mahomed. Here digitalis does no good, but harm; for the arteries are already full to the risk of apoplexy. Indeed this last accident has followed the administration of digitalis under these circumstances. The full artery, then, is a contra-indication, just as much as an empty artery is an indication for the administration of digitalis, whether the heart be diseased or not.

Digitalis is a diuretic, says another: "Whenever the bulk of urine rises then I know digitalis is doing good." Certainly, if a horse be yoked to a cart previously stationary, and after that the cart be seen

moving away, it is a pretty accurate inference that the horse is drawing the cart. The bulk of urine, as Traube taught, is the index of arterial fullness. When the arteries are filled by the action of digitalis the bulk of urine is increased. The rise in the bulk of urine tells in the most unmistakable manner that the action of the drug is filling the arteries. In dropsy, when the bulk of urine is low and the specific gravity is high, then digitalis is preëminently useful. When albuminuria is present from venous engorgement in heart failure, the administration of digitalis will often be followed by its disappearance. As the arteries are filled the veins are depleted; the albumen, which tells of venous congestion, disappears as this state of the veins is relieved; as the arteries are filled the bulk of urine rises.

The great matter for the practitioner to remember about digitalis is, that it increases the energy of the ventricular contractions; and that the clinical indication for its administration is an empty artery. With such view before his mental vision the practitioner will rarely experience any difficulty in deciding when to give, or when to withhold the potent digitalis—potent for good or harm according to the circumstances under which it is prescribed.

In cases of cerebral anemia digitalis may often be prescribed with advantage when it is desirable to raise the blood-pressure within the arteries.

Irrigation of the Large Intestine in Dysentery.—Professor J. T. Whittaker, of Cincinnati, in a recent clinical lecture, published in the Cincinnati Lancet and Clinic, took as his text an alarming case of dysentery, which he had treated successfully by irrigation of the large intestine, after ipecac—in which he has great faith—and other means had failed. He said:

Injections of nitrate of silver, to each pint of water one half dram, were thrown into the rectum, and the first injection of three pints held the disease in abeyance for two days. The dysentery returned, however, and then alum injections, one dram to each pint, were administered with most beneficent effect. No recurrence of the attack since then has followed. The patient is now cured; she has but one natural operation a day. She is taking the fluid extract of eucalyptus for its tonic effect, and is feeding on milk and animal food. Injections in this way are of modern use. Nitrate of silver has been especially thus recommended, but alum has the advantage of being less dangerous and less costly. The effect is possibly due to the flushing out of

the bowel, by means of which the germs of the disease with the decomposing detritus of tissue are all carried away. Carbolic acid was recommended once for the same purpose, but is objectionable from the fact that where there is ulceration of the bowel carbolic-acid poisoning may be set up. In its place, however, salicylic acid has been used, being less dangerous, besides deodorizing the stools, which are generally indescribably offensive in dysentery. The large bowel holds six imperial pints; we succeeded in injecting only three pints, and had to desist on account of pain; the injection is allowed to escape at once. In a case of acute epidemic dysentery, first, if possible, give large doses of ipecac—twenty to thirty grains every four hours. If this should not quickly be followed by any benefit, have recourse to these injections.

The patient though cured of the first attack is subject to relapses caused by errors in diet and change in weather.

Should symptoms of dysentery again appear we would give ipecac again, but in smaller doses—one half to one grain every three or four hours alone, or with bismuth or soda, or both. Of course, a mild case of sporadic dysentery would call for nothing but light laxation. Remember only and always that even the most ancient authors knew and said that dysentery was a disease which constipates the bowels, and hence always avoid astringents.

Treatment of Gonorrheal Cystitis.—Dr. P. Geffrier writes, in the *Revue de Chirurgie*, that the treatment of gonorrheal cystitis consists in the local application of a two-per-cent solution of nitrate of silver to the diseased mucous membrane. The instruments employed are a flexible catheter with an olivary point of the size eighteen or twenty (Charrière)—unless a stricture compel the use of a smaller one—and a small Auel's syringe. The catheter is attached to the nozzle of the syringe, and both are filled with the solution. The instrument is then oiled and passed up the urethra until the bulbous extremity is felt to enter the bladder. It is then slowly drawn back until the point is arrested at the entrance of the urethra. When in this position the piston of the syringe is pressed upon and a few drops of the nitrate of silver solution are slowly dropped upon the mucous membrane of the neck of the bladder. For the first injection, fifteen drops of two-per-cent solution suffice. This amount is

subsequently increased to twenty, thirty, or even fifty drops. The injections are to be repeated every other day. The pain caused by the application is slight and but of short duration. Internal treatment by copaiba or sandalwood oil may be conjoined with the local application, but it is not necessary. The injections should be continued as long as the frequent desire to urinate exists, even though all the other symptoms have subsided; otherwise there is danger of a relapse. In some severe forms of gonorrheal cystitis the inflammation is not limited to the neck of the bladder, but involves, in greater or less degree, the entire vesical mucous membrane. In these cases it is advisable to wash out the bladder thoroughly with a solution of nitrate of silver of one part in five hundred, in place of the local application of a stronger solution. The injections should be made soon after the patient has passed water, in order to avoid the precipitation of the silver by the chloride of sodium in the urine. (Medical Record.)

EXCESSIVE EATING AS A CAUSE OF DISEASE.—Dr. Liveing, one of the first of dermatologists, says in the London Lancet:

I frequently meet with cases of intractable eczema pudendi in women past middle life, of sedentary habits, and eating three large meat meals a day, and trying by all means in their power to stimulate their appetite, under the erroneous impression that they are "keeping up their strength." Now, in these and similar cases, medicine and local treatment are almost equally useless, unless there is at the same time a thorough reform in the diet. The first point is to deprive the patients of sugar as an article of food, except just enough to make light puddings palatable. The reason for this is that much of the sugar passes the liver unchanged, and is therefore worse than useless as food. The next point is greatly to reduce the animal food, especially mutton and beef, and to substitute for it simple clear soup, and poultry or fish in moderate quantity once a day. Lastly, the chief part of the daily diet should be made up of light, farinaceous, and milk food, such as bread, rice, and macaroni. This is, I know, contrary to the view often entertained, that saccharine urine should be treated by an animal diet, and that starch should be as much as possible excluded. Now, whatever good may result from such a diet in

some cases, I am quite sure that it does not answer in those to which I refer; on the contrary, exactly the reverse holds, and the old routine practice, except so far as sugar is excluded, is quite wrong. I have seen the sugar disappear from the urine and the eczema depart under a change of diet such as I have above recommended. The truth is that many people at sixty, when the tissue changes are slow, eat as much or more than they did at twenty, when all the processes of change are at the height of their activity; what wonder, then, that unnatural work is thrown upon the skin, kidneys, and other excreting organs of the body. There is some substantial truth in the saying that small eaters live the longest.

Sulphate of Atropia in the Treatment of Coryza.—According to Dr. Gentilhomme, sulphate of atropia (from a quarter of a milligram to one milligram, given as a pill, say one half minim to two minims of the *liquor*) has an immediate effect in the first stages of coryza, often arresting the progress of the disease. It also produces great relief when the coryza is confirmed, but its action is less remarkable than at the beginning of the inflammation. When bronchitis exists at the same time, the sulphate produces an equally favorable effect upon the bronchial mucous membrane. The employment of sulphate of atropia is based upon the fact that it has the power of lessening the nasal mucous secretion to the extent of complete arrest; and at the same time it acts beneficially upon the vessels by relieving their congestion. (*Rev. Méd. Francaise et Etrangère.*)

Operation for Varicocele.—Mr. Barker, in the Lancet, describes an operation for varicocele, which he had performed successfully in three cases. The operation is but radical, with the antiseptic precautions, causes less trouble to the patient than the ordinary methods. The skin of the scrotum was thoroughly cleansed with a five-per-cent carbolic lotion, as also all instruments and the surgeon's hands, no spray being used. The scrotum was then pinched up between finger and thumb in the usual way, so as to include the veins and exclude the vas deferens; it was then notched with a scalpel, and through the

opening thus made a needle bearing a medium-sized twisted silk ligature (previously soaked for about an hour in the same carbolic solution) was passed. The veins were then allowed to slip backward, and the needle was made to carry the silk forward again through the same puncture, but this time in front of the veins. The latter were thus, of course, included in the two loops of silk leaving the scrotum by the same aperture. The ends of these were now tied tightly over the veins about one eighth of an inch apart. They were then cut short and allowed to slip into the scrotal tissues. Every thing was in the mean time protected from any contamination by frequent wiping with a carbolized sponge. A little padding of salicylated wool was the only dressing.

Treatment of Venereal and Common Warts.—Prof. Unna recommends for the treatment both of venereal and of ordinary warts the continuous application of unguentum hydrargyri, containing five per cent of arsenic. In the case of a young girl, on whose hands were a hundred or more warts, the application for three weeks of a plaster, containing in each 0.2 square meter ten grams of arsenic and five grams of mercury, caused entire disappearance of the disease without any irritation of the healthy skin. Cure was effected not by necrotic destruction of the warts, such as occurs in the use of caustics, but by resorption, as in cases of spontaneous cure. (*Monatshefte für Prakt. Derm.*)

Corrosive Sublimate in the Treatment of Gonorrhea.—Dr. Liestikow (*Deutche Medicinal-Zeit.*) thinks he has confirmed by a series of experiments the discovery, made by Neisser, of the presence of a special form of bacteria in gonorrheal discharges. In the first stage of a gonorrhea, when the discharge is thick and abundant, but few of the bacteria can be seen. They exist, however, in great numbers in the thin and scanty secretion of the later stages, sometimes even when the disease has existed over a year. In the treatment of gonorrhea the author employs an injection of corrosive sublimate, which Koch has found

most fatal to the various forms of bacteria. He uses a solution of one part to twenty thousand, one in ten thousand being found to be too irritating. In private practice a still weaker solution of one part to thirty thousand is employed. The injections are made three times a day, and should be continued for three or four days after all discharge has ceased. The bacteria disappear, or are greatly diminished in number, after one day's use of the injections, but return again if the latter are discontinued too soon. Treatment by injections should not be begun until after the acute inflammation has subsided. (New York Med. Record.)

PILOCARPINE IN POLYURIA.—A patient suffering from polyuria azoturica used belladonna, bromide of potassium, laudanum, injection of morphine, and electricity without effect. (*H. Morgagni Giornale.*) Hypodermic injections of nitrate of pilocarpine, 0.20 in water 20.0, diminished the daily secreted urine from ten litres to two litres, and the quantity of urea was reduced from nine grams to three grams. The weight of the body increased to eight kilograms in two months' time. In polyuria glycosurica the sugar disappeared after a short time. Fifteen injections will generally cure these diseases. (New York Medical Record.)

A NEW MERCURIAL FOR HYPODERMIC USE.—After several years of experimental and practical trials, Professor O. Liebreich has at length devised a preparation of mercury which is especially serviceable for hypodermic use. He announced his discovery at the recent meeting of the Berlin Medical Society. The name of the new compound is formamid of mercury, or *hydrargyrum formamidatum solutum.* Liebreich has found that about thirty injections of a one-per-cent solution suffice for ordinary cases of syphilis. Given internally, the drug is inert.

Notes and Queries.

SIR THOMAS WATSON, BART., M.D., F.R.S.—The following appreciative notice of the good baronet and illustrious physician, prepared by Ernest Hart, Esq., the very distinguished editor of the British Medical Journal, we copy almost entire:

Few men in any profession have descended to the grave honored, beloved, and respected in a higher degree than Sir Thomas Watson. Throughout a career of professional activity, prolonged to the utmost limits which the conditions of life allot to man, he attracted to himself admiration, regard, and respect—admiration of his rare combination of mental qualities and gifts, of his wide professional knowledge and attainments, his power of exposition as a lecturer, his keen clinical insight and practical sagacity, his rich and readily available stores of well classified experience, his accuracy of thought and felicitous clearness of expression, his success as a practitioner, his brilliant powers as a teacher, and his unrivaled faculty of smooth, apt, and copious diction as a writer; regard and respect for his serene and gentle temper, his modest dignity, his benevolent kindness, his unfailing clearness of judgment in the complicated relations of the life of a physician in large practice, and, again, as a professional leader to whom reference was constantly made in the troublous questions of professional etiquette and professional policy, as the head of a great college distinguished for its conservative traditions, for its reputation for learning, and for the important interests under its charge, and passing through periods of active development and critical change. Singularly accessible to all who sought his age or his advice; averse, from natural tenderness, to inflict pain, or even a shade of disappointment, by word or deed; with a mind peculiarly open to argumentative convic-

tion, and of that thoughtful cast which saw quickly the objections to, as well as the reasons for the conclusions which were pressed upon him, Sir Thomas Watson possessed a character and a disposition which did not allow him to pass through professional life wrapped up in his own peculiar work and satisfied with attention to the merely scientific details of his profession, or the rigid performance of his own special daily duties, and the fulfillment of his own personal ideal of work or of happiness. Nothing that happened in the professional world of human or scientific importance was alien to him, and there are few men among his contemporaries who have not at one time or another come to him for advice and guidance, and of all who came there is perhaps not one that left without a feeling of increased confidence in his elevated judgment, in his great knowledge, and in his singular wisdom. Conciliatory to the utmost bounds of kindness, he was never open to the charge of favoring compromise, and thus he retained in a peculiar degree the confidence and respect of all classes of the profession. In him the College of Physicians found a leader never unmindful of its dignity, but sensible of the importance of changes in its constitution which many thought revolutionary. The consulting physician saw in him a typical representative of the dignity of the class; the general practitioner recognized also a sentiment of professional fraternity and a consciousness of the claim of universal brotherhood of medicine, which kept always in the foreground that basis of democratic equality which is the keystone of the heart of professional strength and unity. It is rare indeed to find any man of whom it may be said as of him that there is not one man in the profession who would at any time have declined to have accepted Sir Thomas Watson's judgment on any personal or professional question as final. His sense of justice, his habitual reference to all questions of detail to unassailable principle, his flexibility of mind, and his quick perception of character gave him a rare but well-justified ascendancy over even the ablest of his cotemporaries; and while Sir Thomas Watson practiced his profession he was, during a long period, recog-

nized without dispute as its leader. During the later years of his life, and long after he had retired from practice, he continued to display a keen interest in professional affairs, and was still an eager student of its literature. The opinions which he formed were always provisional—formed upon the best evidence then available, but subject to revision. The last edition of his celebrated Lectures testify to his rare gift of judicial impartiality and to the admirable candor and philosophic modesty with which he revised and altered the conclusions of earlier years, and the unfaltering courage with which he avowed such changes of opinion. Among the most notable instances of such change were the new convictions which he accepted as to the change of type in disease and as to the pathology of cholera. In both instances he had watched with careful study the progress of medical knowledge, and in neither did he hesitate, at the close of the controversies to which they gave rise, to declare himself convinced in a sense contrary to his former opinion, and to set forth with the utmost clearness and graceful simplicity the new conclusions to which he had been led. Still more recently he took occasion to study afresh the relation of vaccination to smallpox, in the light of the discussion which took place at the Conference of the British Medical Association, the subject of animal vaccination, and subsequently to avow in these columns his conversion to the opinions which we had advocated as to the relation of vaccine lymph to cowpox lymph; to withdraw the opinions expressed in his lectures, and to accept the conclusions which we had advocated.

In Sir Thomas Watson the Association loses one of its most revered and respected members, and one of its warmest friends and admirers, and we lose a most frequent correspondent, one to whose unfailing kindness, to whose generous aid, to whose eloquent pen, and to whose friendly correspondence we have for many years owed personally a debt of gratitude and admiration.

Sir Thomas Watson's private letters were models of epistolary composition not only in their singular elegance of expression and their aptness and felicity of thought, but conspicuously

in the unfailing courtesy, in the unassuming, unaffected, and natural modesty, and in the generous kindness of the language in which they were invariably couched. The gentle sweetness of expression, the modest dignity of demeanor, the intelligent kindness which beamed from every feature translated the natural character of the man; and it will be long before the recollection of so sincere a friend, so perfect a gentleman, so accomplished a physician, and so true a councillor will fade from the recollection of those on whose minds they have been impressed by personal intercourse.

The late Sir Thomas Watson was born on the 7th of March, 1792, at Montrath House in the parish of Broadhembury, Devonshire. In 1811 he was admitted to the University of Cambridge, where he took the degree of B.A. as tenth wrangler, in January, 1815, and in the course of the next year took the degree of M.A. He did not begin the study of medicine until the somewhat late age of twenty-seven years, when he commenced his studies at St. Bartholomew's Hospital under the late Mr. Abernethy, who was his friend as well as teacher. During the session of 1820–21 Mr. Watson prosecuted his studies at some of the medical classes in the University of Edinburgh, and then returned to Cambridge, in which university in 1823–4 he held the office of junior proctor. In the following year he took the degree of Doctor of Medicine, and married the daughter of Edward Jones, Esq., of Brackley, in Northamptonshire, whom he had the misfortune to lose five years after their marriage, and three days subsequently to the birth of their second child. Soon after his marriage Dr. Watson commenced practice as a physician in the same street (Henrietta Street, Cavendish Square), though not in the same house, in which he lived fifty-seven years. In 1826 he became a Fellow of the Royal College of Physicians, and in the following year, on the resignation of Dr. Southey, was elected Physician to the Middlesex Hospital. In 1828, when University College was opened, and during the following two years, Dr. Watson, as Professor of Clinical Medicine, in 1831, gave lectures on the cases of disease

which came under his care in the wards of the Middlesex Hospital. He resigned the Chair of Clinical Medicine in 1831, and was appointed Professor of Forensic Medicine in King's College.

In this year his first contribution to medical literature appeared in the Medical Gazette, entitled "Remarks on the Dissection of Bishop, and the Phenomena attending Death by Strangulation." The notorious Bishop murdered an Italian organ-boy, and then took his body to the dissecting-room of King's College for sale. He was subsequently hanged for the crime, and his body was sent to the same institution for dissection. Hence Dr. Watson's lecture on the subject. From that time Dr. Watson was a frequent contributor to the Medical Gazette.

Dr. Watson's private practice had continued to grow and prosper, and a proof of the estimation in which he was held is found in the circumstance that to him was intrusted the care of Sir Walter Scott on his last voyage from London to Edinburgh. In 1836, on the resignation of Dr. Francis Hawkins, Dr. Watson was appointed Professor of the Principles and Practice of Medicine, and while performing the duties of this position he first delivered, during the session of 1836–37, those *Lectures on the Principles and Practice of Physic* which take first rank among the standard classics of medicine. It may be convenient to our readers to be reminded that these lectures were originally published week by week in the Medical Gazette. The first lecture appeared on September 25, 1840, and the last of the series on September 23, 1842. These lectures have for nearly half a century held their ground as the classical authority in the science and practice of medicine in all English-speaking countries. Like Graves and like Trousseau, he added the charm of eloquence to the solid merits of extensive learning, great clinical experience, sound judgment, and fertility of resource in the expedients of therapeutic practice. The language he used was clear, simple, and familiar; full of apt illustration and happy example, well balanced, marked frequently by striking and unexpected turns of thought, and disdaining neither touches of pathos nor of humor to enforce a conclusion or to illustrate an

opinion. Many a student, taking up the volumes of lectures with ominous anticipations of weary and toilsome reading, has rejoiced to find them as attractive as a romance and more instructive than a manual.

Sir Thomas Watson's funeral, which took place on Friday, December 15th, was attended by a representative gathering of his professional friends; but it will be no exaggeration to affirm that, notwithstanding his advanced age, which daily brought the event nearer to the minds of men, the death of Sir Thomas Watson has left a sense of individual loss and grief in the minds of the profession at large, even in those who only knew the lamented Nestor of the medical profession through the medium of his immortal writings.

We have selected the subjoined correspondence as a good illustration of the reluctance with which Sir Thomas Watson allowed himself to be drawn into any thing like controversy, and, when circumstances necessitated such a course, of the mingled gentleness, firmness, and authority with which he expressed himself. The circumstances are as follows:

Shortly after Mr. Liston's death a letter appeared on December 18, 1847, by Dr. C. J. B. Williams, headed "The Physical Signs of Disease in the Case of the late Mr. Liston." Dr. Williams says that, having seen and heard it stated in many quarters that there were no physical signs of disease detected in the chest of his lamented colleague he thought it right, for the credit of physical diagnosis, as well as in justice to himself, to make known the result of his own examination of the case as recorded in his own note-book. This was to the effect that he had noted a "marked dullness above the left clavicle and scapula (on strong percussion), large tubular breathing and voice-sound in the same space, tubular respiration above upper inner angle of right scapula." This, in connection with his previous disease, he considered "most alarming;" but he adds, "it saves me from self-reproach that I never said any thing to countenance his disposition to make light of his malady, but uniformly asserted my conviction of its serious character." Dr. Williams visited Mr.

Liston the second and last time in November, and expressed in his letter his regret that Mr. Liston did not follow his advice "in any particular," but "got relieved by strong exertion in riding a restive horse, which promoted expectoration." He adds, "after this I did not see my professional friend, as he placed himself under the care of physicians who both before and after this period found no physical signs of disease, and who therefore took a more favorable view of the case than I did. The result is known, and I make no further comment on it."

Dr. Watson replied, calling attention to the injustice done to him and Dr. Forbes by the insertion of the last paragraph, and pointed out that Dr. Williams was "quite wrong in supposing and stating" that they (Dr. Watson and Dr. Forbes) had found no physical signs of disease and had taken a more favorable view of the case than Dr. Williams had; that, on the contrary, both he and Dr. Forbes had been throughout aware of the symptoms stated in Dr. Williams's letter. Dr. Watson pointed out that the whole tenor of Dr. Williams's letter was such as to lead the unacquainted to infer that they had not done as he would have done, warning the patient, and "countenanced his disposition to make light of his malady." Dr. Watson concluded by saying: "Was it courteous or even fair to publish these statements without previously ascertaining from one or the other whether the facts of the case really were as you understood them? Would it have been charitable or generous so to exhibit our mistakes, even if you were sure that we had made them? Do you indeed believe that if our lamented friend had been intrusted solely to your care, and could have been induced implicitly to obey your directions, the fatal 'result' of this disease would have been prevented? One more question I venture in perfect amity to propose for your calm consideration. Is it consistent with your character and your high rank in our profession, with your office (which presents you as an example to so many) in one of our great metropolitan schools of medicine, thus publicly and needlessly, under profession of a zeal for science, to proclaim your own superior sagacity and (by implica-

tion) the comparative ignorance or unskillfulness of others, your contemporaries pursuing, in the same place, to the best of their humbler abilities, the same vocation with yourself, and, in this instance, engaged in the peculiarly anxious duty of ministering to the relief of a professional brother? Would Baillie or Heberden have done this?"

This brought a letter from Dr. Williams retracting the passages complained of, and, finally, Dr. Watson's "letter of satisfaction":

Dear Dr. Williams—I thank you for your candid, temperate, and satisfactory letter of explanation. If (as I am glad to know from your assurance) I misconstrued the meaning and spirit of your letter in the Lancet, my excuse must be that I did so in common with every one of those who have spoken to me about it, and they have been many.

Indeed it was the interpretation upon it by some of my friends that first brought the letter under my own notice. It was especially the paragraph which you so frankly retract, with the addition of the next little sentence—"the result is known, and I make no further comment on it"—that (as it seemed to me) gave force and point to all which had preceded. But for this paragraph I should not have thought of troubling you with any expostulation on the subject. I assure you that I did not know, until I saw it so stated to you in the Lancet, that "Mr. Liston had first sought your aid," or that he had formally consulted you at all. On the very morning of the hemorrhage he sent me a message simply requesting that I would call on him. I did so on my first going out, and found him recovered from the faintness produced by the loss of blood. But I was not then nor at any time informed that he had previously sent for you. I became aware indeed, at a much later period, that his chest had been once examined by yourself, as well as by another physician, also his colleague in University College. But I believed that these examinations had been casually made upon some occasion of your officially meeting together. Had I known that Mr. Liston had desired your counsel in the first instance, I should have been, not willing merely, but anxious, in a case so painfully responsible, to obtain the comfort and advantage of your valuable assistance.

Let me assure you, finally, that if, writing to you as I did upon the spur of the occasion, I transgressed the just limits of self-defense, or so expressed myself as to cause unnecessary pain to your feelings, I am sorry for having done so. I trust also, and on my own part am assured,

that what has occurred in this very distressful matter will not be suffered to impair the mutual respect and good will which had hitherto subsisted between us.

I remain yours, truly, THOMAS WATSON.

Of late years, when he had more leisure in the mornings, he was always pleased to receive a visit from any friend, with whom he would talk over the past, brimming over with humorous anecdote referring to his personal experience of men and manners in general. For some time back he was often found, toward the latter part of the morning, reposing on a couch with a small jug of fresh milk ready by his side. He was eminently social in his tastes, and remarkably attached to his children and his four grandchildren, whose photographs and artistic handiwork he prided himself on pointing to on the walls of his consulting room, where, amid books and papers, was the cast of the large bust of Esculapius which exists in the British Museum.

DR. CARPENTER ON HUMAN AUTOMATISM.—The subject discussed was the automatism of those perceptive processes by which are determined the distance, direction, and notably the solid form of external objects. In us this is acquired instead of being congenital as in the lower animals. Many observations of infants and of adults who have recovered sight by means of the removal of the lens (cataract) prove the truth of this statement, and show that a combination of the impressions received from several senses is necessary in order to create the ideas we possess of external objects. A person blind from birth, upon obtaining vision, will not recognize forms like the cube, sphere, and pyramid, notwithstanding the sense of touch has long since made them familiar. Locke doubted this, and the question, then unanswerable, since his time has received repeated answer. For example, a young man whose sight was restored in one eye was unable by vision to distinguish a cat from a dog, both of which he was accustomed to fondle. He finally taught himself to know the cat by handling her frequently, meanwhile looking at her intently. A remarkable case was that of a child aged four

years, blind from birth, who recovered sight by the operation for cataract. He gave evidence of his ability to see. The operation occurred in a house in which the boy had become familiar by feeling his way about. After the operation, while going over the house he at first felt and looked as he went, but for some time evidently preferred the guidance of touch, and eventually began to find his way by sight alone. Upon returning to his home, however, he was quite unable to discover his way by sight among objects perfectly familiar to him, and for a long time closed his eyes in going from place to place. But when taken to a place entirely strange to him he used his sight without embarrassment, and finally overcame the difficulty in using sight at home.

In a third case a blind seamstress obtained her sight, and when shown a pair of scissors could not recognize them without touching them. These examples indicate the amount of education necessary in the training of our automaton, which, once disciplined, subsequently makes no mistake unless something is wrong in the mechanism.

Passing to the automatisms of the higher intellectual processes, the lecturer discussed those uniformities which, some being general in the race, others being the result of special training, and already studied under the name of "laws of thought," markedly illustrate the doctrine of automatism as well as the power of the will to devote itself to such processes as may be chosen. Wordsworth has testified that his best poetry was created by allowing his mind to direct itself intently upon all the phases of his subject, and then waiting for a spontaneous outflow of poetical imagery from his mental mechanism. Mozart, whose musical faculty had from childhood been trained with most assiduous care, had only to think out the general plan of a composition, deciding as to the place to be given to solo, recitation, duet, quartette, etc., and then allow his thought to work of itself and evolve its own results. In the same way trained mathematicians solve difficult problems.

But most instructive of all is the action of memory. We

endeavor to recall some half-forgotten fact, name, or date. After fixing the attention upon the subject for a certain length of time and recalling every accessible circumstance, we find it better to withdraw the attention, to "hang up" the subject, and leave the matter to time. The general result will be a sudden return of the missing fact to the consciousness. In all these cases of "unconscious cerebration" it is noteworthy that we must first give direction to the process, and, moreover, that in order to obtain results we must previously train the automata.

Evolutions of the judgment are also common under the same conditions of a previous training, antecedent fixing of the attention, and, as far as possible, a voluntary development of the process. Thus an executor, unable to decide as to the best solution of a difficult provision in a will, arranged a plan which, though unsatisfactory, seemed the most feasible, and then dismissed the matter from his mind. Some days later, on waking from a sound sleep, a perfect plan flashed into his consciousness, and was accepted without change. Experience has shown that these automata, once trained, work far better when left alone than when the attention is fixed upon them. When we apply the theory of automatism to our beliefs, there arises the question as to whether we are responsible for them, and whether we can believe what we wish to believe. The reply is, that if we have accustomed ourselves to give due weight to all the evidence which may affect our conclusions, we are not responsible for our belief. We may, however, close our mental eyes to certain aspects of the case, just as an unjust judge may refuse to admit evidence on one side and unduly admit it on the other. In such case we are directly responsible for what we believe.

An Important Medico-Legal Decision.—Judge Campbell, of the Supreme Bench of the State of Michigan, has given a decision of interest to the medical profession. A surgeon being called in consultation to a case of compound fracture of both legs below the knee, advised amputation of both extremities, which was refused. One leg was amputated, and the other

finally recovered with deformity. The plaintiff sued for his pay, and the defendant claimed malpractice. This decision of the Supreme Court establishes the following points: There is no presumption of law as to the value of a surgeon's services, nor that a jury can ascertain their value without testimony from persons knowing something about it. Nor has a jury a right to reduce the compensation claimed for such service where undisputed testimony shows it to have been appropriate, and on their own unsupported notions that the treatment adopted should have been different.

A jury has no right to ignore testimony that has not been discredited, and form independent conclusions, without testimony, on matters that require proof beyond their conjectures or opinions.

The fact that a surgeon changes a course of treatment adopted by another does not in itself show that the former course of treatment was not proper at the time; nor is the patient's failure to recover perfect soundness of limb in itself evidence of malpractice; nor is the fact that he survived, although he refused to allow a particular course of treatment, evidence that such course might not have been proper under the circumstances.

The jury in an action for the value of surgical services has no right to find malpractice without testimony from persons who are qualified to give opinions on the methods of treatment.

AN ECONOMICAL PILL.--The "everlasting pill" was composed of metallic antimony, which was believed to have the property of purging as often as it was swallowed. This was economy in right earnest, for a single pill would serve a whole family during their lives, and might be transmitted as an heirloom to their posterity. We have heard of a lady, who having swallowed one of these pills, became seriously alarmed at its not passing. "Madam," said the physician, "fear not; it has already passed through a hundred patients without any difficulty."